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HERBERT S. LANGFELD

PRINCETON UNIVERSITY

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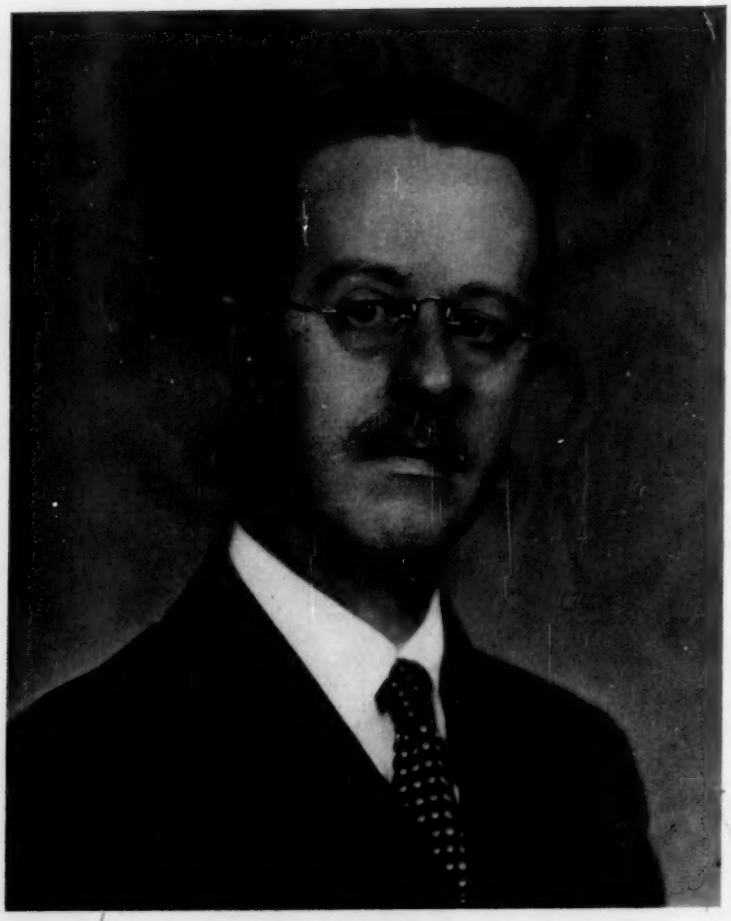

Edward S. Robinson

1893–1937

VOL. 44, No. 4 JULY, 1937

THE PSYCHOLOGICAL REVIEW

EDWARD STEVENS ROBINSON

Professor Robinson died in New Haven on Saturday, February 27, 1937 in his forty-fourth year, following cerebral injuries received three days previously. The many instant expressions of grief from University colleagues, from citizens of New Haven, and from an unusually wide circle of friends and colleagues throughout the country gave spontaneous evidence of love and esteem for this human, buoyant, and brilliant personality. Such expressions of sorrow followed hard upon those for Mr. Robinson's wife, Florence Richardson Robinson, who had died a scant three months earlier. Their relations were peculiarly close, and Mr. Robinson has in his writings paid repeated tribute to the inspiration of his wife's help.

Mr. Robinson was born in Lebanon, Ohio, April 18, 1893. He received the A.B. degree at the University of Cincinnati in 1916, the A.M. in 1917 at the Carnegie Institute of Technology, and the Ph.D. at the University of Chicago in 1920—one of the many significant psychologists who studied under James R. Angell. He came to Yale as instructor in 1920 and after but a year there was regretfully released to become assistant professor at the University of Chicago; the associate professorship followed in 1923. In 1921 he married Florence Richardson, also assistant professor at Chicago. In 1926 he was again invited to Yale, as visiting associate professor, and was a visiting lecturer at Harvard in 1927. In the fall of that year, at the unusually early age of thirty-four, and looking younger, he settled at Yale as professor.

Mr. Robinson's swift advancement was but fitting recognition of his rapid academic accomplishment. Extra-University solicitations of his services became, too, increasingly

numerous. Treasurer of the American Psychological Association, 1925 to 1930 (and later on its Council, 1934–1937); editor of the *Psychological Bulletin*, 1930–1934; cooperating editor of the *American Journal of Psychology*, 1925–1935; an editor of the *Journal of Social Psychology*, 1935–; and, at the time of his death, vice-president (and chairman of Division I) of the A. A. A. S., chairman of the committee of the N. R. C. on Personality in Relation to Culture, Resident Fellow of Timothy Dwight College (Yale), Lecturer since 1932 in the Yale School of Law, chairman of the Division of General Studies of the Graduate School, on the council of the Adult Education Association, and active in the American Association of Museums. A varied, revealing constellation of services.

The titles of Mr. Robinson's writings, 1919 to 1936, number sixty-three, including six books and some hundred and thirty-five pages of an unfinished manuscript of a general text on Social Psychology. Sixteen of the publications were under joint authorship, chiefly with Mrs. Robinson. A mere chronological grouping of the titles would throw sharp light on the temper of Mr. Robinson's mind. Almost all of the score or more of articles in the experimental field fell in the period 1919 to 1927, at the University of Chicago, where he won his spurs as an experimentalist and teacher. These laboratory contributions—chiefly in the general fields of learning and mental work—have, as is well known, become an integral part of current psychology. Soon after reaching Yale it became obvious that his interests were swerving rapidly toward the larger social settings of psychology. The slant is easily discernible in earlier writings, but the drive of his essentially explorative mind to reduce the untried to empirical control rather than to linger in the safer security of what had already been charted, became more and more pronounced. His initial social area of investigation was the "display" and educative functions of the Public Museum. Field work started in 1935 and there followed, 1928 to 1936, some half dozen articles of his own, with others by co-workers whom he had trained.

Along with these studies he promptly began to exploit the social-psychology motif in other ways. For the first time significant courses in social psychology were given to undergraduates at Yale, and an elective graduate seminar, the topic changing from year to year, soon fixed itself as a broadening course not to be missed, and drew students and colleagues from other fields. Doctoral problems in social psychology likewise became a wholesome addition to laboratory problems. In Law Mr. Robinson saw an unusually rich field. In 1932, with easy competence, he assumed direction of a somewhat novel venture in legal education—a first year course in the School of Law on professional practice and legal ethics. "It is," he remarked, "great fun." This work intensified his interest and enlarged his insight into the possibilities of a psychological jurisprudence and, with a brilliant and congenial soul, Thurman W. Arnold, Professor of Law, a joint seminar was soon under way in the Law School (open also to selected graduate students of psychology) on "The psychology of modern judicial and legislative institutions." This too was not only "great fun" but according to students one of the most enlightening and exciting educational experiences that they had ever had. It had already resulted before Mr. Robinson's death in perhaps his most significant, and surely his most provocative book ('Law and the Lawyers', 1935). Mr. Arnold's equally provocative companion piece, 'Symbols of Government'—in which "my principal indebtedness is to Edward S. Robinson"—appeared at about the same time.

With his interest in general social psychology, in Museum work, in jurisprudence still undiminished Mr. Robinson assumed, also in 1935, as chairman of the Division of General Graduate Studies, direction of a project, actively sponsored by Yale's progressive Graduate Dean, to provide suitable training for competent individuals already professionally employed, and not candidates for a Ph.D. degree, in whatever subjects their work required, regardless of University departmental lines of division. This was a project not to train investigators, but better to prepare those persons already in vocational or professional pursuits. It was a prompt

success. This year the group of students included a newspaper man, an orchestra leader, museum workers, and an art critic.

To Mr. Robinson these specific fields figured chiefly as empirical samplings for exploratory try-outs of psychological methods in the entire social milieu, and such wider implications color the publications that began soon to be issued. Museums were samples of other public institutions, the Law of other intrenched but inadequate social ideologies. General Studies, as he once excitedly remarked, "opens up the whole big field of adult education." Six of the publications related to Museum work, three to Law, nine to problems involved in General Studies and Adult Education. Scattered articles also appeared on other matters of social-psychological concern—social work, senior college education, the influence of radio on its 'fans,' the voter's mind, and propaganda—not to mention the developing integration of his accumulated ideas in the incomplete manuscript on general social psychology. His was indeed a many chambered mind.

It is not necessary to summarize the specific contributions of Mr. Robinson to psychology. They are readily accessible. Nor is this the place to attempt evaluation. But it is the place to venture some further hint of the general ideology of the man. For few dwelt more habitually among ideas. After Chicago, personal laboratory work—but not the interest—practically ceased. It is the other ideas, I feel, that he himself would particularly wish emphasized. He once quizzically described himself to me as 'a sort of roving professor.' I am inclined to think that the happy hormonic activity implied in the term would have been increasingly characteristic in the years ahead.

Psychology, he felt, had been realistically alive, neither to its opportunities, nor to its plain duties. While nothing human should be alien to it, it had too often permitted traditional theory and method to define its problems, instead of the reverse. To stand on the demand for quantitative precision, for instance, inevitably excludes problems crying for investigation, or leaves them to unripe 'opinion,' which,

none the less, becomes an operating motive in practical behavior. The locus of such problems is chiefly in the social and personality fields. The pressure of the factual situations there should define the pertinent methods of approach, and these are multiple and do not shake down into any single 'best.' Eventual precision, yes, but to seek, in the present state of affairs, nothing else is to subordinate adequacy to system—an age-old failing. He held these views not defensively, but with a sort of tolerant militancy. William James, whom Mr. Robinson greatly admired, had this insight for individual psychology fifty years ago; a like mélange of procedures is indicated now in the newer frontier of the social field. Those who knew 'Robby' well sensed not merely an admiration for James, but similarity of flair.

These general insights Mr. Robinson would have been the last to regard as complete novelties. His originality lay in reducing them to a various and particularizing applicability. As felt convictions they pervaded all his thinking and doing; and he went about, with refreshing disregard of criticism, showing that and how they could be utilized empirically in specific, indicatory areas of human concern. "It's working, *isn't* it," he not long ago smilingly remarked when some judge had chidingly said in reviewing 'Law and the Lawyers' that he was too hard on both; "that's what I want them to say."

The fluid unconventionality of Mr. Robinson's thinking emerges in many ways. An early distrust of 'compartmentalizings' of all sorts developed, as he recurrently met their refrigerating influence, into an almost crusading ardor. His major venture into systematic psychology ('Association Theory Today') was a deserved *Ehrenrettung* of a discarded, but still highly relevant concept, in the face of its modern, more alluring dresses, such as the conditioned reflex. ("There are today too many psychologists who stand in awe before the educated salivation of a Russian dog.") The same motive had operated in his attempt to inject a realistic psychology into that "logic tight compartment," that "vested interest in old ideas, the Law," and into public institutions like the Museum, with their ingrown ideologies. This same motive underlay

his enthusiasm for breaching the academic barriers between disciplines in the interests of General Studies and Adult Education. But it is when a distinction is boosted into a symbol, 'system,' or school of psychology with exclusive proprietary rights—and for a criticism of this process he reserves his best felicities of style—that Mr. Robinson's sense for plastic relevance is most outraged. The very titles of two of his articles are evidence—'Behaviorism: l'enfant terrible' and 'A Little German Band, or The Solemnities of Gestalt Psychology.'[1] It was his keen historical sense, which ran as a steadying accompaniment through whatever he wrote or discussed, that was largely responsible for the common-sense balance of his appraisals. He had wide reading and an unusual memory, of a sort that readily yielded the apt recall.

Mr. Robinson would of course dislike any label for his psychology. But that his scientific outlook was pervadingly a functional one is by this time abundantly clear. His alertness to functional relevance permeated also his practical doings. His seminar in psychological jurisprudence was given in the Law School, not in his own Department; all but one of his articles on education, museum work, and other social topics were published in non-psychological journals. It was, in short, the 'excitatory tendencies' of workers in the practical fields that he sought to influence, for to him the hope of liberalizing the psychological approach lay in demand and pressure of the factual problems in those fields, not within academic walls. In his texts and lay writings he had, too, an engaging, informal way of clarifying the elusive, the intricate, the technical, because he possessed a well nigh faultless insight into the 'apperceptive systems' of his readers. His University classes, particularly his advanced ones, were events. As in much of his writing, his main interest was to make the men aware of the factual problems and of the functional techniques that might be realistically applied to them.

[1] See further his good and good-natured review of the 1929 'edition' of Gestalt (Köhler) in *J. Genet. Psychol.*, 1930.

It would be hard to find another single individual who could with his ready mastery do the various tasks that have been described as his. For men with philosophic grasp, scientific method, empirical sense, imaginative insights into the investigable realities of complex social settings, all combined with human charm, are very rare. That Mr. Robinson made the impression on psychology that in so few years he did make, with the ripeness of full maturity still ahead, only intensifies the tragedy of his death.

To those who knew him well, 'Robby' was a term that held a multiplicity of endearing traits. They are understandingly suggested in an editorial of the *New Haven Journal-Courier:* "Professor Robinson's colleagues have said in formal statements how eminent he was in the scientific and professional fields. . . . But the deeper man is understood by his common intercourses. . . . He had that rarest of manly virtues, an utterly winsome and graceful temper. His wit played about the edges of every controversy, darting now and again to its heart with an observation that brought not merely laughter but light. Fencing across a table on any subject under the sun, he was vigorous, but did not wound; where other men find it enough to inspire respect, he invited affection too. . . . Professor Robinson's loss to science has been called irreparable; and those who knew him would almost think that loss the least of the deprivations in his death."

ROSWELL P. ANGIER

YALE UNIVERSITY

THE SEARCH FOR CERTAINTY[1]

BY HARVEY A. CARR

The University of Chicago

Under this title I propose to discuss what I regard as an illegitimate method of measuring reliability in the field of learning. I refer to the method of measuring reliability by correlating the scores of the even and odd trials, and of assuming that the degree of reliability varies directly with the size of this correlation.

This method was borrowed from the field of mental tests. The mental testers early raised the legitimate question of the reliability of their test measures, and the method of correlation inside of a battery of test items was devised to answer this question. Correlations may be obtained between the scores of the first and second halves of the items or of the odd and even items. It is assumed that the degree of reliability varies with the size of the obtained correlation. Any discussion of the logical validity of this method in the field of mental tests will be postponed for the present.

The method was introduced into the field of learning in the following manner. Paterson had been brought up in the field of tests, and later came into contact with some maze experiments with animal subjects. Naturally there arose in his mind the question of the reliability of these learning scores, and the possibility of measuring it by the inter-correlation of trials in the place of test items.

Acting upon this suggestion, Hunter secured records upon a number of problems, and computed the correlations between the even and odd trials. He found that the size of the correlation varied with the problem. The maze correlations were extremely low, and hence we have the well known dictum that the maze is an unreliable instrument of measurement.

[1] Presidential address delivered before the Midwestern Psychological Association, Urbana, Illinois, April 24th, 1937.

In spite of considerable criticism, this method of measuring reliability has been widely adopted, especially by comparative psychologists. At the present time, no article in the field of comparative psychology is considered complete without a numerical statement of the reliability of its data.

Hunter employed animal subjects, and he was primarily concerned with the maze problem. I wish to call attention to the fact that the same question of reliability and the same method of measuring reliability are just as applicable to the entire field of learning—to the records of both animal and human subjects, and to the acquisition of acts of skill and the memorization of verbal materials. It is this wider question in which I am interested, and I shall discuss this method in terms of its applicability to the entire range of learning data, though for convenience I shall generally use the maze for illustrative purposes.

Hunter borrowed this method from the mental test field, and simply transferred it to the field of learning without once raising the prior question of its applicability to this new situation. It is this question which Hunter ignored that I propose to discuss, *viz.*, the validity of this proposed method of measuring the reliability of learning data.

To what extent are our learning scores reliable, and to what extent are they unreliable? Are these scores reliable in proportion to the size of the correlation between even and odd trials, and are they unreliable in so far as this correlation is less than 100? If we obtain a correlation of 75, can the two values of 75 and 25 be regarded as rough measures of their relative reliability and unreliability respectively? The problem is here stated in very general and abstract terms. For an intelligent discussion of the topic, the problem must be stated in more specific and concrete terms. For this purpose, let me make the following series of clarifying statements.

1. The problem can be stated as follows: To what extent can we rely or depend upon these learning scores? This form of statement immediately suggests the obvious fact that these numerical learning data have no reliability apart from the uses to which they are put. A set of numbers in and

of themselves are neither reliable nor unreliable. Their reliability is entirely a function of the purposes for which they are used, and their reliability must be evaluated in terms of these uses. 2. Learning scores are used for at least three purposes. The individual scores are used as measures of certain features of the animal's behavior in solving the problem, *i.e.*, as measures of *learning performance*. A group of individual scores are sometimes used as measures of individual differences in *learning ability*, and two sets of such scores are often used to measure the influence of some *experimental variable* upon learning performance. 3. There is thus no *single* problem of reliability. These scores are not 'either reliable or unreliable' as the proponents of the method seem to assume. There are three problems of reliability involved, and the reliability of these scores must be separately evaluated for each of these purposes. It is entirely possible for a set of scores to be highly reliable as measures of learning performance, and yet to possess but little or no reliability as measures of individual differences of ability. 4. It is now evident that the concept of reliability is concerned with the *accuracy* of these measurements. These scores are reliable in proportion to the accuracy of these measurements, and they are unreliable in so far as they are subject to error. The three problems involved may now be stated as follows: To what extent can we rely or depend upon these scores as accurate measures of learning performance, as accurate measures of individual differences in ability, or as accurate measures of the influence of some experimental variable upon learning performance?

I shall first discuss the reliability of learning scores as measures of *learning performance*. I run a group of rats on a maze problem, and secure for each a time and error score. These scores purport to measure two observable features of the animal's behavior while traversing the maze. As we all know, these quantitative evaluations of the animal's behavior are subject to considerable error. How can we detect and measure the amount of error involved in these scores? Can we use the proposed correlation method for this purpose? Is

the reliability or accuracy of these measures proportional to the size of the correlation, while the amount of error present is denoted by the deviation of this value from 100? Such a doctrine involves the assumption that all errors that are present are of such a kind that they invariably operate to decrease the size of the correlation. The question can best be answered by considering each type of error separately.

1. The scores may not be reliable because they were faked in whole or in part. An experimenter is confronted with the task of comparing the average performance records of 6 groups of 50 animals each. Let us suppose that he runs 30 animals in each group, notes the general trend of the results, and in the interest of economy of time and effort supplies the remaining 20 records for each group out of his imagination. Obviously these records are not accurate nor reliable measures of group performance.

2. The records may be intentionally fudged in many ways. Our experimenter obtains records of learning performance for the 50 animals in each group, and he usually finds some erratic and irrational deviations from a consistent trend. He now intentionally alters or discards some of these records in the interest of consistency and uniformity.

3. I may now mention that much more insidious source of error—the unintentional distortion of our measurements by our scientific interests and prejudices, and who can assert that he is wholly immune to such an influence?

How can we detect the presence of and measure the amount of these three types of error? Can we use the method of correlation for this purpose? One must admit that the presence of these errors will probably affect the size of the correlation. However, they may operate to raise the correlation as well as to decrease it. What is to prevent an intelligent faker or fudger from arranging his data so as to secure a correlation of 99? Surely we can not regard this method of correlation as a new type of honesty test, or as a new form of lie detector.

The only method of detecting these types of error is to check up the experimenter with an honest observer, and this

method is obviously impracticable. The only way to prevent these errors is to do as we have always done—attempt to impregnate our students with the ideals of scientific integrity.

I raise this question of the integrity of the experimenter not only because of its relevance to the topic, but also because I have sometimes wondered as to the degree of our success in inculcating this trait. Presumably any major distortion of records is a rare occurrence, but I venture to state that most of us have encountered some gossip in respect to such alleged cases.

However, minor distortions—both intentional and unintentional in character—are a different matter. We are forced to recognize the fact that scientific integrity is not an innate trait. It is a virtue that must be assiduously cultivated. We must train our students as best we can, accept their data on faith, and frankly recognize the fact that our confidence may at times be misplaced. No department can guarantee the reliability of all of the experimental data that emanate from its laboratory, and no department is justified in using this correlational method as a test of the integrity and reliability of its workers. For my part, I much prefer to put my trust in the integrity of the average experimenter than in a thousand of such correlations irrespective of their magnitude.

4. Time records are usually taken with a stop watch. These records may not be strictly accurate, because our watch may be running too fast or too slow. These errors can not be detected or measured by the method of correlation, for we are here dealing with *constant* errors—those that are present in each of the trials of all of the subjects. It is quite obvious that this class of errors can not materially affect the size of the correlation, and certainly we can not use the size of the correlation as a means of measuring the influence of a factor that does not affect that correlation. The only method of detecting and measuring these errors is to do as we did with our Hipp chronoscopes—check their reliability in terms of some standard timing device.

No experimenter is the least concerned with this possible source of error. We are not primarily interested in the

absolute accuracy of our time measurements. We are concerned with the question as to whether our obtained time differences are sufficiently large to be decisive. Our differential time results may be sufficiently accurate and reliable for the purposes of the experiment, although our measures in absolute terms may be subject to a considerable amount of constant error.

5. Two observers may take records of the same group of rats. Their error records may differ materially in absolute amount because they used different criteria of an error. In spite of the differences in their records, these two observers may work up their respective data, and obtain the same conclusions and the same alleged coefficient of reliability.

These illustrations suffice for the formulation of three propositions concerning the relation of errors of measurement and the size of the correlation. 1. All constant errors—those present in each of the trials of all of the subjects—can not influence the size of the correlation, and hence they can not be measured by this device. 2. All errors of measurement that are intentionally or non-intentionally made in the interest of attaining a given end will affect the size of the correlation, but they may operate to increase as well as to decrease it. Obviously this method can not be used to detect and measure these sources of error. 3. Only those errors that exhibit some chance distribution will necessarily decrease the size of the correlation, and hence the method is only applicable to their detection.

There are several sources of error that may be listed under the third category. (1) Errors may be made in the transcription and computation of the data. Even the alleged coefficient of reliability is subject to this type of error. The incidence of these errors is materially reduced by the use of computing machines. However, the only way to detect and eliminate this type of error is the device of repeated computation. (2) The number of errors in any trial may not be accurately counted. The enumeration of errors in the initial trials when the animal is likely to work by fits and starts is peculiarly subject to these inaccuracies. Such mistakes are

materially reduced by the use of mechanical counters. (3) Finally, there is the difficulty of using any criterion of error in a consistent fashion. A particular deviation of behavior may be labelled an error at one time and not an error on another occasion. These inconsistencies in the method of counting errors are usually limited to the first part of a problem, for it is a matter of common observation that the experimenter usually settles down to a consistent schema as the experiment progresses.

Can we use the coefficient of correlation to measure the influence of these chance errors of measurement? Obviously we can not do so unless we also know what would be the size of the correlation if these errors of measurement were not present. We must first be able to answer the following question: What other groups of determining factors are present, and what size of correlation will they mediate? A discussion of the nature of these residual factors and their influence on the size of the correlation will be postponed for the present, but I may state that their combined effect is of such a character that the problem of measuring the influence of these chance factors on the basis of the correlational data is an insoluble one.

The method thus can not be used as a valid test of the reliability or accuracy of our measures of learning performance. I am now perfectly willing to admit that I do not think that the adherents of the method ever proposed, or would even attempt to defend, its use for this purpose. Neither do I believe that the mental testers have ever used the method for this purpose. I have here discussed the method in relation to this type of reliability, not to belabor a man of straw, but in order to evaluate it for all possible uses to which it might be put.

The second problem of reliability involves the use of learning scores as measures of *learning ability*. For an intelligent discussion of this problem, we need to define the concept of ability as related to that of performance. Ability is *indirectly* measured through performance scores, but not all performance scores can be taken as measures of ability.

Performance scores can be used as measures of ability only when they are taken under certain specific conditions. I shall illustrate these propositions by using speed of typing as an example. Speed of typing is a function of 5 groups of variable factors, and this classification has a value only for the purpose of this discussion.

1. Speed of typing is a function of a number of objective factors. One can type familiar connected material at a faster rate than a similar amount of tabular material. One can type two pages at a faster rate than 100 pages of similar material. One can usually type from memory faster than from dictation. A quiet environment is conducive to speed of typing as compared with a distractive environment. Speed may vary with all of the variable objective conditions under which the act occurs.

2. Speed also varies with the motivation and the volitional attitude of the subject. An individual can write at an habitual level, and then materially increase this speed by attempting to do his best, and again this speed may be increased to some extent by appealing to the motives of rivalry and monetary rewards.

3. Speed may be affected by many of the temporary and pronounced alterations of the mental and organic condition of the subject, such as the effect of drugs, fatigue, the debilitating effects of a prolonged sickness, loss of sleep, and an emotional disturbance. The influence of these variable factors is obvious.

4. Speed is also subject to a wealth of chance or uncontrollable factors—both objective and intra-organic in character. The degree of effect of these factors will probably vary with the individual and the act in question. They will probably exert but little influence upon the typing performance of a trained subject as compared with that of a novice. They will probably exert less influence upon typing than upon maze performance. It is a common belief that maze learning performances are peculiarly subject to such influences. It is for this reason that the solution of a maze problem has been characterized as a trial and error performance with a *chance*

success. In other words, the time and error scores of each trial are very largely determined by this chance group of factors.

5. Speed of typing is also a function of some of those relatively permanent and enduring conditions of the organism that characterize a given individual and differentiate him from his fellows. I shall hereafter refer to these conditions as 'constitutional' factors, and they may be either innate or acquired in respect to origin. For example, the difference in speed scores of one pair of individuals may be largely due to differences in training, while the differential scores of another pair of individuals may be mainly due to anatomical and physiological conditions that are innate in origin.

Ability is a relative term. The differences in ability of a group of individuals are measured in terms of that distribution of performance scores that is known to be a function of the fifth group of factors—the constitutional differences of these individuals. Individuals differ in ability in so far as their performances are functions of these constitutional differences.

The isolation of the influence of these constitutional factors is usually accomplished by the method of exclusion. We measure typing ability by comparing the performance scores of a group of individuals under experimental conditions in which we know that the influence of the first four groups of factors has been excluded. Under these conditions, we therefore conclude that the obtained distribution of performance records is due to the fifth group of factors, and we utilize this distribution of scores as measures of the relative amounts of ability possessed by these individuals.

In measuring typing ability we secure performance records from a group of individuals upon the same kind and length of material and in the same environmental conditions, so that the obtained distribution of records can not be due to differences in objective conditions. We attempt to secure equality of motivation by instructing each subject to do his best and by enlisting his coöperation to this end. The possible influence of the third set of conditions is eliminated by excluding from the test all individuals who are suspected of being under the

influence of drugs, undue fatigue, prolonged loss of sleep, or a profound emotional disturbance.

The usual device for detecting the presence of and eliminating the influence of the chance group of factors, is the method of repeated administration of the test over some interval of time. The test is repeated once, and if the correlation between the two administrations approximates 100, the influence of the chance factors is regarded as negligible in amount, and the obtained distributions of scores may be used as indices of ability. A low correlation, on the other hand, indicates that the chance factors are influentially present. In this case, the influence of these factors is balanced by securing the average score of a number of repeated administrations distributed over some interval of time, and it is these average scores which must be used as indices of the relative ability of the individuals in the group. The number of administrations necessary to secure an effective balance is obviously a function of the degree to which these factors influence the records.

This concrete definition of ability in terms of its relation to performance is essential to a consideration of the proposed method of testing the reliability with which performance scores can be used as measures of ability. We shall first discuss the use of the method in the field of mental tests. We secure performance records from a group of children on a battery of test items under the same objective surroundings. We secure equality of motivation by instructing the children to do their best and enlisting their coöperation in so doing. All subjects are eliminated who are obviously afflicted with undue physical or mental disturbances. In so far as the test conditions are successful in eliminating the influence of the first three groups of determining factors, the obtained distribution of performance records must be explained in terms of chance factors and the constitutional differences of the individuals.

The usual procedure of eliminating the influence of the chance factors by the method of repeated administration is impracticable on several counts. The interval between

successive tests must be sufficient to obviate the possibility of a memory connection. If the chance factors exert a considerable influence upon the records, a large number of repetitions are necessary to secure an effective balance. A test is primarily a device to secure a quick and easy diagnosis of an individual even at some sacrifice of accuracy. The method of repeated administration thus defeats the essential purpose of a test.

The experimenter is thus confronted with the problem of evaluating the relative influence of these two factors on the basis of a *single* administration. It was for this purpose that the method of inter-item correlation was devised. The logic of the method is obvious. If the distribution of the performance scores for each of the items of the test is wholly a function of the group of chance factors, the obtained correlation should approximate zero. If the obtained distributions are entirely a function of constitutional differences, and in so far as the various items test the same thing, the correlation should approximate 100. When both factors are operative, an intermediate correlation will be obtained. If a correlation of 70 is obtained, the relative influence of the chance and constitutional factors may be roughly denoted by the values of 30 and 70 respectively. The obtained correlation thus denotes the degree of accuracy with which the distribution of performance scores may be used as measures of the relative ability of the members of the group, and the larger the correlation the greater is the degree of accuracy.

The method may now be applied to the maze problem. A group of rats is tested on the same maze pattern and under the same objective conditions. The experimenter attempts to secure equality of motivation. Only animals in good physical condition are used. The rats are tamed and habituated to the apparatus, and care is exercised to prevent the incidence of any emotional disturbance during the process of learning. The influence of the first three groups of factors in accounting for the distribution of the learning scores is excluded. This distribution must be explained in terms of the

last two groups of factors—chance and the constitutional differences of the individuals.

The method of eliminating the influence of chance factors by repeated measurement encounters greater difficulties than in the field of mental tests. We can not duplicate this learning process until the effects of the initial mastery of the maze have been entirely eradicated, and by this time we find that our group of rats are tottering on the verge of the grave. The permanence of the learning effects, the restricted longevity of rats, and the degree to which the maze performance is subject to the chance group of factors are insuperable difficulties. We can eliminate the factor of longevity by employing human subjects on a stylus maze, but it would be practically impossible to keep control of a group of human subjects for the required period of time, even if we assume that the effects of learning are ever completely lost.

Presumably the method of correlation was applied to learning for the same reason it was used in the field of mental tests, *viz.*, to measure the relative influence of the two factors within a single group of learning data. The logic of the method is again obvious. If the distribution of the scores for each of the trials is entirely a function of chance factors, the inter-trial correlation will approximate zero. If the distribution is wholly a function of the constitutional differences of the individuals, the correlation will approximate 100. If both influences are present, an intermediate correlation will be obtained, and the size of the correlation can be used to denote the degree of accuracy with which these performance scores can be used as measures of the influence of constitutional differences, or the accuracy with which the learning scores measure individual differences in learning ability.

For purposes of critical evaluation, let me restate the nature of the problem and the proposed method of solution. In both the mental test and the learning situations we obtain a group distribution of scores. We know that this distribution of performance records is to be explained in terms of two groups of factors—chance and the constitutional differences of the individuals. We are thus confronted with

the problem of measuring their relative influence on this distribution, or, more specifically, with the problem of isolating and measuring the degree to which this distribution is a function of the constitutional factors. We now compute the inter-item or inter-trial correlation. We know that the size of this correlation is also a function of the same two factors. We also know that the influence of these two factors is *opposite* in character. The constitutional factors tend to give a perfect correlation, while the chance factors operate to decrease it. The size of the obtained correlation in reference to zero and 100 is thus a measure of the relative influence of these two opposed factors, and we now assume that these correlation values will also measure the relative influence of these factors in accounting for the group distribution of records.

Let me now call attention to the fact that the logical validity of this method is based upon a *tacit* assumption. It is based upon the assumption that the size of the correlation is an *exclusive* function of these two opposite influences. In other words, it is based upon the tacit assumption that there is no *third factor* present which will account in part for the size of the correlation. If the correlation is a function of but two opposed influences, this value may be used to differentiate between them. However, if the size of this correlation is a function of three or more factors, the problem of differentiating between them on the basis of this correlational data is obviously an insoluble one.

I am now perfectly willing to admit the truth of this assumption so far as the field of mental tests is concerned. However, I wish to call your attention to the fact that such a third factor is necessarily present in every learning situation, and we here encounter the fundamental fallacy involved in applying this method to learning data. What is a learning process? It is a process in which the performance of an individual on the tenth trial is a function of the preceding trials. It is a process in which the individual's performance changes from trial to trial in virtue of the preceding trials. It is a process in which the scores for the successive trials are

so causally tied together that the scores for the odd and even trials must necessarily exhibit some sort of a correlation with each other. In all learning situations, the size of the obtained correlation must thus be a function in part of what I shall hereafter term the 'learning factor.'

We know that the performance in any one trial does influence the scores of all the subsequent trials, but we do not know the mechanism nor the amount of this influence. We here enter the field of speculation, and I suspect that this mechanism is quite complex in character. Let me suggest two possibilities. 1. Due to chance influences, a rat on its first trial may become confused and lost in an intricate section, and run up a high error score as a consequence. This attitude may become relatively fixated and persist to some extent in the second trial. 2. Another rat may also enter this section. He may succeed in preserving his orientation while thoroughly exploring it, and thus practically master it in the initial trial. In so far as the relative strength of these perseverative and eliminative tendencies remains constant from trial to trial, and from animal to animal, a high correlation will be obtained, while any variation in their relative strength will decrease the size of the correlation. These suggestions serve to emphasize the complexity of the interrelationships involved, and they point to the fact that the cumulative effect of these influences upon the size of the correlation is indeterminate in the present state of knowledge. This learning factor will mediate some sort of a correlation—either large or small, and I suspect that its size will vary considerably with the problem and the kind of subject employed.

The size of the correlation is thus a function of at least three variables. The factors of chance and learning are known to be operative, but the amount of their influence is unknown. Consequently the problem of isolating the influence of the constitutional factor on the basis of the correlational data is insoluble. I may now note that the existence of this last factor is purely hypothetical. We do not know that the constitutional differences between rats are

sufficient in character or amount to influence the relative size of the learning scores, until the existence of this influence has been demonstrated by other means. For all we know, the size of the obtained correlations may be entirely a function of the first two variables.

In the field of learning, we systematically study all possible factors that may influence the learning scores. In memory, we have studied the influence of such objective factors as kind and length of material and its spatial and temporal organization. In animal learning, we have a very extensive series of experiments that are concerned with motivation and incentives. We have extensively studied such variable organic factors as the effect of drugs, fatigue, loss of sleep, muscular tension, *et cetera.* I wish you to note that these three groups of experiments are not concerned with the problem of individual differences in learning *ability;* they are concerned with the relation of these variable factors to learning *performance.* Individual differences in ability is the object of study only when we attempt to isolate and measure the influence of the constitutional group of factors. The great majority of all learning experiments thus deal exclusively with *learning performance.* Only a limited number of experiments in both the human and animal fields have ever been concerned with the problem of individual differences in learning ability, and so far as I know only two types of experiment have ever succeeded in demonstrating the existence of individual differences in maze learning ability.

If the method of correlation was designed for the purpose of testing the reliability of our scores as measures of ability, its use should be exclusively limited to those few experiments which deal with this topic. As you all know, however, the method has been indiscriminately applied to the whole gamut of learning problems—those dealing with performance as well as with ability.

This indiscriminate use of the method in actual practice immediately raises the question of the purpose for which it is used when the experiment is not concerned with the problem of ability. Let me cite a concrete example. Let us investi-

gate the influence of some objective factor upon learning performance. To do so, we compare the learning records of two groups of individuals. All of the objective conditions are alike for the two groups with the exception of the variable factor which we are studying. The two groups are equated in respect to motivation and attitude. All individuals are free from any undue mental or physical disturbances. The influence of chance factors is equated by using groups of sufficient size to secure an effective balance. Finally the two groups are equated in learning ability by one of the devices that are used for that purpose. Ability is not the object of study in this experiment. We are studying the effect of this variable upon learning performance. There are but two possible uses to which the correlation can be put in this experiment. (1) It may be used to test the reliability with which these scores measure learning performance, and (2) it may be used as a test of the reliability with which the group records measure the influence of the objective factor on learning.

In the first case, we encounter the first problem of reliability which we have already discussed. As we previously noted, all constant errors of measurement do not affect the size of the correlation. The influence of one group of errors upon the correlation is indeterminate in direction and amount, and one group of errors tends to decrease the size of the correlation. However, the size of the correlation is in part a function of the chance factors affecting performance, it is in part a function of the learning factor, and it may be in part a function of the constitutional group of factors. There are too many variables involved to permit the isolation of any one on the basis of the correlational data. The method can not be validly used as a test of the reliability of the learning scores as measures of learning performance.

With the second alternative, we encounter our *third problem* of reliability. To what extent can we rely upon the obtained difference between the two groups of learning records as an accurate measure of the influence of this objective variable which we are studying? Can we use the size of the

even and odd correlation for this purpose? There are two such values—one for each group. Which one shall we use? We again encounter the same difficulty as before. The size of the correlation is a function of too many variables. Even if we ignore these difficulties, we still can not use the correlation for this purpose for the simple reason that this objective variable will not affect the size of that correlation. This objective variable whose influence we are studying will presumably affect the score for each of the trials of all of the individuals, and any such constant factor will exert no effect upon the correlation of the odd and even trials. Obviously, we can not use the correlational value as a means of measuring the influence of some factor that does not affect that value.

The method has absolutely no value for any one of the three problems of reliability, and any conclusions based upon it are utterly fallacious in character. In other words, this proposed method of measuring the reliability of learning data is itself *wholly unreliable*, and I now wish to call your attention to the fact that its continued use may do *irreparable* harm. Let me cite two illustrations.

In the literature, one finds a number of experiments dealing with each topic in the field of learning. The several experiments dealing with the same topic usually differ materially in respect to their conditions, and as a consequence different conclusions are obtained. The reviewer is confronted with the task of evaluating these experiments in the attempt to bring order out of this apparent chaos. The usual procedure, which I think is wrong, is based upon the assumption that only one of these conclusions can be true, and hence the reviewer is confronted with the task of deciding between these rival claimants for the throne. We are all familiar with one type of evaluation employed. The first article was published many years ago, and hence it contains no coefficient of correlation. The reliability of its data is thus open to question, and hence its conclusions may not be true. The second experimenter obtained a low correlation, his data are unreliable, and therefore his conclusions are wrong. The third man secured a high correlation, his data

are reliable, and hence his conclusions are accepted as the truth for which we are seeking. You will note that in actual practice this correlational value tends to become a criterion of truth and error. This feature of the method should certainly be called to the attention of those of our philosophical friends who are interested in this problem of epistemology.

Let us suppose that one investigates the effect of some objective variable upon learning performance. His integrity and reliability as an experimenter is unquestioned. He is meticulously careful in respect to all controls, and his measures of performance are just as accurate as it is humanly possible for them to be. He happens to obtain a low coefficient of correlation, and his numerical results indicate that this factor is not effective on learning performance. A second experimenter believes that this factor is efficacious, and repeats the experiment in order to confirm his belief. He obtains a few records under the same conditions, and then fakes the remainder of his records in such a way as to prove the efficacy of this factor, and also to secure a high coefficient of reliability. These two experiments are found in the literature, and our reviewer is confronted with the task of evaluating them. The outcome is quite obvious. The method may operate to discard truth on the one hand and to perpetuate rank error and fraud on the other, and in the realm of science what greater harm can any method do than this? The continued use of a method that is known to be wrong is just as serious a scientific crime as is the deliberate faking or fudging of results, and I am making these statements for the benefit of a few individuals who have admitted that this correlation possesses but little or no value as a measure of reliability, but who have yet continued to use it.

Any method that is both erroneous and harmful in character should be immediately discarded, but I am not any too hopeful. I suspect that there are those who may admit the logical validity of this analysis, but still remain but half convinced, for how can any method be entirely wrong that is mathematical in nature? This delusion that any psychological conclusion that is derived from mathematical opera-

tions must perforce be true is apparently so widespread and so insidious in character that it deserves some comment.

Mathematical operations give us nothing but mathematical truths. Mathematical operations never transcend the field of mathematics. To attain any psychological conclusion from mathematical data, we must *interpret* those data in terms of the purpose and the conditions of our experiment. In making this interpretation, we at once leave the field of mathematics, and enter that of psychology. The use of mathematical data in the solution of any psychological problem does not guarantee in any manner the truth of our interpretation of those data. One can attain either true or false conclusions from the same mathematical data according to the character of the intervening activities of thought.

In making these general statements, I am enunciating no new doctrine. I am just repeating what our statisticians have been preaching for several years. I merely propose here to cite the use of this correlational method as a concrete and pertinent and horrible example of the truth of their assertions.

What does this correlation measure? We often hear this question asked, but seldom answered. This correlation is a direct mathematical measure of the degree of concomitant variability exhibited by our two sets of records. It is a direct measure of the consistency of our individual records—of the degree to which an individual who makes a good score on the odd trials will also make a good score on the even trials. When we have made these two statements, we are through with mathematics, and are confronted with the task of psychological interpretation.

The size of this correlation is known to vary from problem to problem, and consequently its size must be a function of certain causal influences that are present in our experimental situation. It is a truism that we can *indirectly* measure causal influences by *directly* measuring their effects. We indirectly measure heat by directly measuring the expansion of our column of mercury. Likewise we can use this direct measure of the concomitant variability of our two sets of scores as an indirect measure of the relative influence of its

causal factors, *provided* that the causal relation involved is of such a character to permit it. If this correlation is wholly a product of the two opposed influences of chance and constitutional differences, we can use the correlational data as an indirect measure of the relative influence of those two factors, or we may legitimately conclude that the accuracy with which our distribution of learning scores can be used as a measure of individual differences in ability is proportional to the size of the obtained correlation. Since our analysis of a learning situation has shown that this correlation is a product of at least four causal factors whose influence is indeterminate in amount, the problem of isolating the influence of any one of them on the basis of the correlational data is insoluble.

The fallacious use of this method is due to the fact that its adherents have failed to make an adequate interpretative analysis of the causal significance of this correlation in terms of a *learning situation.* I am not indicting this movement for the poor quality of its interpretative thinking, but rather for the apparent absence thereof, for you will note that the proponents of the method merely borrowed it from the field of mental tests, and then blindly, unreflectively, and indiscriminately applied it to the whole field of learning on the basis of a *superficial resemblance* without any attempt to evaluate it in reference to that situation. Psychology is engaged in the search for that illusive factor of reliability, but I can assure you that we will never attain that objective by the simple procedure of using a calculating machine as a substitute for brains.

Why do we use this method? Is it wholly due to ignorance, and is Plato right, and will we immediately discard the method when we are *intellectually* convinced of its fallacious character? Perhaps so, but I have my doubts, for I suspect that its banishment will still encounter a considerable degree of 'mental resistance' and arouse some occasional twinges of regret. Must we not desert Plato, and turn to Freud for a more adequate account of human motivation?

We here enter the field of speculation, but I like to speculate, even though I do not always give much credence to the

resultant product. However let me at least recount some of my speculations concerning the motives that actuate a psychologist at work, and I may as well admit that some of these speculations are probably 'introspective' in character.

I suspect—in fact I quite strongly suspect—that many of us eagerly adopted this method, really like to use it, and will be averse to giving it up, simply because it is mathematical in character. I suspect that many of us are somewhat inclined to use mathematical operations, whenever it is at all possible to do so, in the fond hope that our experiments will thereby become suffused with some of the dignity and respectability of that science. I suspect that many of us are somewhat inclined to use mathematical operations, whose nature we do not fully understand, as a means of compensating for the well known inferiority complex of our science. Human nature seems inclined to regard with awe all those things which it does not understand, and hence I have occasionally suspected that some unsophisticated individuals may be naively inclined to regard mathematical operations as some sort of a mysterious device that enables one to pull truth out of an empty hat. I have also entertained the suspicion at times that the modern psychologist turns to mathematics for much the same reason that our forebears were accustomed to satisfy their cravings for authority and finality by appeals to the holy writ. I have also heard the opinion expressed on a few occasions that some instances of the use of mathematical operations can best be classified under the rubric of 'exhibitionism,' and I am presuming that this term was used to denote those alleged cases in which those operations did not contribute either to the clarification or the solution of the problem. Let me also call your attention to the fact that a well known economist,[2] in a recent article, went one step further and asserted that some instances of the use of mathematics in his science have merely served to *confuse* the issues involved. I am not passing judgment on these charges, but surely we should avoid all suspicion that we are merely doing mathematical stunts as a means of gratifying non-scientific motives.

[2] Stephen Leacock, Through a glass darkly, *The Atlantic Monthly*, July, 1936.

I shall not add to this list of possible motives, but I shall again ask—why did we uncritically adopt and use this method —and it may be well to envisage this question against the psychological background of the time. This was the period in which there flourished our 57 varieties of psychology. It was a period of intense individualism and carping criticism in which no self-respecting psychologist could think of agreeing with any of his fellows if there was any possible way of avoiding it. Yet it was during this time that the method was introduced, and we find that the voice of criticism was at once stilled, and that the method was almost immediately and universally adopted. Why did we do this? Surely there must have been some reason for our actions, and I have made the somewhat humorous suggestion that our acts were dominated by a mathematics complex—by an itching urge to run correlations without much regard to their significance.

I suspect, however, that the illicit use of mathematics for such purposes is not our only scientific sin. Did we not once go chemical and atomic, much to our later embarrassment? We have also gone biological and physiological in turn, and I hear that we are now attempting to solve all psychological problems in terms of *physical* analogies. Have we not all been inclined to use shining instruments of brass and nickel that are much more complicated and precise than our experiments warrant, and have we not sometimes even chosen our experimental problems primarily to indulge our whim for mysterious and impressive types of instrumentation? To what extent have we done all of these things in order to convince ourselves and others that we are being much more scientific than we really are? We psychologists are scientific, in my opinion, but like chemists and physicists we are also human beings, and I can not refrain from speculating at times at the wealth of essentially human motives that might be brought to light if all our scientific endeavors were subjected to a rigorous Freudian analysis. Frankly I should not much care to serve as such a subject.

Irrespective of motivation, I think that we will all admit that psychology should seek for the assurance of the certainty,

and reliability, and the worth of its findings. These are all worthy aims, but in my opinion the continual search for these virtues in foreign fields is doomed to failure, for I am sure that we will find them, if ever, only in the rigid logic of our own scientific procedures. We psychologists should strive to make our science respectable, but we must first achieve self-respect, and to do so we must cease from relying upon all extraneous props, and learn to stand squarely upon our own scientific feet.

In conclusion let me state that I am not attacking the mathematical movement in psychology. I have considerable respect for that movement, and much hope for its future accomplishments. Psychology will always offer many opportunities for the legitimate use of mathematical operations, and we will always encounter certain types of problem that perhaps can be solved in no other way. I am here merely engaged in paying my respects to the various illicit uses of mathematics, as exemplified in particular by this wholly unreliable method of measuring reliability, and it is well to note that it is such flagrant misuses of mathematics that are so apt to bring this movement into disrepute. If this movement is to thrive and prosper, all of our amateur mathematicians must learn to recognize the fact that the validity of their mathematical operations and the validity of their psychological interpretations thereof are two wholly distinct problems. Finally let me repeat that the use of valid mathematical operations in achieving a psychological conclusion does not guarantee in any shape or manner the truth of that conclusion, and one might also add that psychological results attained by these operations are, in virtue of that fact, not a whit more true or more important than those achieved by other means.

A CRITICISM OF PAVLOV'S CONCEPT OF INTERNAL INHIBITION [1, 2]

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The concept of the conditioned response, with or without Pavlovian terminology, has become an important consideration in any contemporary theory of the learning process. The major phenomena discovered in the laboratories of Pavlov (**18**) and Bekhterev (**2**) have been verified by many other workers. However, Pavlov's interpretations of some of these phenomena have not met with general acceptance. The specific concept bearing the brunt of attack has been that of 'internal inhibition.' About it Razran (**19**), Guthrie (**11**), Beritoff (**3**), Wendt (**24**), Winsor (**28**), Chappell (**5**), Lashley (**17**), and others have had something derogatory to say.

The term 'inhibition' has found general use in two different contexts. First, it has been used in a descriptive sense to designate a condition of an organism characterized by various degrees of response decrement. Decrement during a competing reaction or Pavlov's external inhibition illustrates this behavioral use of the term. A second connotation of 'inhibition' involves an assumed process or substance affecting the neurones with a resulting decrement in response. In this context, the term is not only descriptive, but explanatory, and it is here that Pavlovian 'internal inhibition' must be classified.

Pavlov regards it as some neural condition which at times irradiates over the entire cortex, and which at other times becomes concentrated in one limited cortical area. It holds an important place in his theories for he uses it to explain experimental extinction of a conditioned response, and to account for negative, differential, trace, and delayed con-

[1] The writer is indebted to Drs. Orvis C. Irwin, Charles Van Riper, Eliot H. Rodnick, and Carl Iver Hovland, for their critical reading of the manuscript.

[2] The substance of this paper was reported at the summer meetings of the A. A. A. S. held in Minneapolis, Minnesota, May, 1935.

ditioned reflexes. Furthermore, the phenomenon of disinhibition, or the reinstatement of an extinguished or partially extinguished response, is supposed to occur through a temporary removal of internal inhibition as a result of some distracting stimulation. Of still further interest is Pavlov's belief that sleep is the result of a general irradiation of internal inhibition.

The present paper is a criticism of this concept. It will attempt to show (1) that the behavior which probably led Pavlov to conceive of a gradually irradiating cortical inhibition may be explained more parsimoniously, and (2) that such behavior is not necessarily a phenomenon of learning but instead is an artifact due to certain experimental conditions.

The first suggestion of the tenability of such a point of view presented itself during an investigation of external inhibition and disinhibition of a conditioned galvanic skin response in human adults (**25**). The subjects reclined on a cot in a relatively dark environment, and a masking tone was applied through a set of head phones. The situation, therefore, was conducive to sleep. In the report of the study the following statement occurs:

> The subjects of this experiment reported somnolence during the extinction series. Although they had been instructed to remain as quiet as possible, their efforts to remain awake resulted in many slight movements and adjustments in posture and a tendency toward sporadic deep inspiration. Almost invariably, an increment in the conditioned responses followed. The proprioceptive sensations arising from these movements and inspirations are believed to have resulted in autonomous disinhibition, and their frequent occurrence to have been directly responsible for the prolonged extinction series. This interpretation suggests that at least part of what is usually termed extinctive inhibition may be explained in terms of a decreased level of tonicity.[3]

[3] Recently the data have been re-read and consistent though variable increases in skin resistance have been found during extinction in eighteen of the twenty subjects. In the light of a recent experiment by Wenger and Irwin (27) which demonstrated that increases in palmar skin resistance are related to muscular relaxation, these data confirm the report of the subjects that much relaxation occurred during extinction.

Observations made during a conditioning experiment with newborn human infants in the Iowa Infant Laboratory (**26**) have redirected attention to the problem. In work with infants the same general picture of sporadic activity described in the foregoing paragraph may be seen. Frequent periods of sleep alternate with irregular breathing and states of activity. Thus there existed conditions of tactual and proprioceptive stimulation comparable to those referred to in the above experiment.

In the first ten days of life, which is the time infants were available, it was found that conditioning did not reach 100 per cent response. Thus there was not opportunity to study extinction or disinhibition of conditioned responses as such. It was with some surprise, therefore, that behavior similar to these two phenomena was noted. Although it has been long known that infant responses are quite variable, it seemed unusual that a subject, when no sign of self-stimulation was apparent, should suddenly not respond after having given several successive responses to the conditioned stimulus. These states of no response seemed to occur when the subject was very quiet, and were frequently followed by sleep. Of equal interest was the observation that conditioned responses first seemed to appear when the infant was very slightly active, and also that it was under conditions of very slight activity that the conditioned response reappeared after having disappeared. The latter fact strongly suggested spontaneous or autonomous disinhibition by tactual or proprioceptive stimulation. Yet, it was occurring here in infants not only before conditioning was complete but before any specific attempt had been made to build up internal inhibition, *i.e.*, to extinguish the response. Two interpretations of these observations are possible. One, in terms of Pavlov's concept of irradiated internal inhibition, would hold that both response decrement and muscular relaxation are the result of a development of cortical inhibition, the dissipation of which would be

In order to determine whether the same would be true of a seated subject one adult was conditioned and readings were taken of resistance during conditioning and extinction. During conditioning, resistance maintained a nearly constant level. During extinction it rose. Therefore relaxation also occurred in the seated subject.

accompanied by the termination of muscular relaxation and the reinstatement of the conditioned response. He states, ". . . in all cases of internal inhibition, . . . drowsiness and sleep are met with continually . . ." (18, p. 251), and further, "The development of inhibition with its ultimate expression in the form of sleep is due to functional fatigue of the cellular structures of the cortex" (18, p. 259).

The other interpretation has not been expressed so succinctly. It is peripheral rather than central in nature. According to it, the magnitude of response of an organism at any moment depends to a certain extent upon the state of tension of the skeletal musculature at that moment. This interpretation will be amplified throughout the paper. According to it, decrement of the conditioned responses in these infants occurred when other conditions had induced relaxation of the skeletal musculature, and when relaxation had been interrupted, the response reappeared.

There is abundant evidence to support such an interpretation. Within the past two decades a considerable amount of work has been done (1) on the effect of changes in muscular tension upon behavior, and (2) on the effect of changes in behavior upon muscular tension. Dodge (6), G. L. Freeman (9), Bills (4), and Jacobson (16) have figured prominently in these investigations. The results have been surprisingly consistent and have produced a body of fact which has been summarized by Freeman (10) in his recent text on physiological psychology. The main generalizations are:

1. Augmented tension, up to a certain limit, results in response increment; beyond that limit it results in response decrement.
2. Decreased tension results in response decrement.
3. Mental work results in tension increment.
4. Cessation of mental work results in tension decrement.

Freeman therefore has been led to make the statement that,

> . . . all response contains two aspects, the exteroceptive-phasic influence and the proprioceptive-tonic influence. . . . Tonic contraction precedes and underlies all

> phasic contraction (10, 458–59). [It] . . . may be called the postural substrate—a general neuromuscular background which operates to facilitate, inhibit, or otherwise modify phasic responses (10, p. 451).

Illustrations of these generalizations are numerous, and have already been summarized by Bills (4) and Freeman (7). A few are cited here. Concerning response decrement, Dodge (6) has stated that during voluntary relaxation there is an inhibition of reflex processes. Jacobson and Carlson (15) have found that the amplitude of the knee jerk is decreased during conditions of relaxation. A familiar example of response decrement is the decreased speed and extent of movement under conditions of rest or relaxation. Recently, Bagchi (1) has experimentally demonstrated with the electromyographic technique that latency of voluntary movement during relaxation is two to four times greater than that during normal posture. Concerning response increment, Tuttle (23) has shown that the amplitude of the knee jerk is increased about ten times over the passive state when the subject is attending to mathematical symbols, and Freeman (8) has completed the picture by showing that increments in quadriceps tension occur during mental arithmetic. Therefore under a condition of hypertension the amplitude of the knee jerk is increased. A familiar example of response increment following tonus increment is the 'handgrip' technique, long used by neurologists in order to augment weak reflexes.

If this collation of experimental results pertained only to response decrement during muscular relaxation, there would be no basis upon which to contradict Pavlov's interpretation. Since, however, it also includes examples of response increment following experimentally induced tension increment, there is excellent ground for maintaining that the state of the peripheral musculature *per se* in some way affects the responses of an organism. Regardless of the causation of a given state of muscular tension, and regardless of how it serves to modify phasic responses, it becomes possible to proceed from a premise about peripheral conditions. Such a premise might be stated as follows:

(*a*) If, in an organism functioning at a level of maximum magnitude of response, there occurs a reduction in tension in the skeletal musculature, there will likewise occur a reduction in magnitude of response.

From this premise it may be predicted that

(*b*) Any condition or set of conditions inducing relaxation of the skeletal musculature in an organism functioning at an optimum level of response, will also induce a decrement in response.

Now we know from Pavlov that relaxation and sleep did occur in his experiments. To quote a passage already partially cited, ". . . in all cases of internal inhibition, . . . drowsiness and sleep were met with continually. In the case of extinction of a conditioned reflex some animals even at the first extinction showed not only a disappearance of the secretory and corresponding motor reaction, but also a great dullness as compared with the normal state of the animal before extinction" (**18**, p. 251). The conclusion may therefore be drawn that

(*c*) At least part of the decrement in response noted in Pavlov's experiments on extinction of conditioned responses may be attributed to relaxation of the skeletal musculature.

Here, then, is the logical exposition of the interpretation which was suggested but not analyzed during the experiment upon disinhibition (**25**).

Since the response decrement which occurs with general relaxation and drowsiness is not confined to any specific response or pattern of responses, the above deduction might be amplified.

(*d*) If there exists a generalized relaxation of the skeletal musculature there will exist a generalized decrement in response.

It is not surprising, then, to find Pavlov saying:

> . . . experimental extinction of any single conditioned reflex results, not only in a weakening of that particular conditioned reflex which is directly subjected to the extinction (primary extinction), but also in a weakening of other conditioned reflexes which were not directly subjected to extinction (secondary extinction). . . . Sometimes second-

> ary extinction reaches a profound degree, involving even the unconditioned reflexes (18, p. 54).

Nor is it surprising that he felt the need of an irradiating general inhibition to explain the phenomena he witnessed.

Now if this peripheral explanation of response decrement in some extinction experiments is accepted, certain questions arise:

1. How is muscular relaxation itself to be explained?
2. Does muscular relaxation account for all of the response decrement in experimental extinction?

The answer to the first question is considered to be without the scope of the present paper. Let it suffice to say that certain factors influencing muscular tension, as well as some of the nerve pathways over which they operate, are known. Since the latter are fairly well localized, there is small need to postulate a vague 'internal inhibition' irradiating over the cortical hemispheres. The second question, however, is essential for the present discussion, and it has already been answered in the negative, for there are repeated examples in Pavlov's own work wherein the response decrement has become specific and is accompanied by no reduction in other responses. It must be concluded, therefore, that

> (*e*) The construction of the original premise is correct, not only in form but in fact. Only part of the response decrement occurring in experimental extinction may be attributed to reduction in the postural substrate.

It follows then that:

> (*f*) Experimental extinction is a two-fold process when muscular relaxation is known to have occurred.

And further,

> (*g*) Experimental extinction of a conditioned response should be possible without the general decrement induced by muscular relaxation.

Since experimental extinction is involved in establishing delayed, trace, and differential conditioned responses in that in the first two the immediate response to the conditioned

stimulus becomes extinguished and in the last the responses to all but a specific conditioned stimulus become extinguished, it becomes possible to predict that,

(*h*) The development of delayed, trace and differential conditioned responses should be possible without the general decrement induced by muscular relaxation.

Data already exist supporting these predictions. Wendt (**24**), in a recent experiment, has presented evidence purporting to show that extinction of a conditioned response is an active process, explainable in terms of competition between reactions systems. His experiment, the subjects of which were monkeys, is briefly described in his own words:

> Each animal is trained to respond to the onset of a tone by opening a drawer to get a piece of food. The animal is confined in a cage with a barred aperture (grille) through which it can reach out to grasp the handle of the food drawer. The grille can be closed by a roller curtain attached just beneath the grille so that raising of the curtain closes the grille. . . . The situation confronting the animal is this: At approximately one minute intervals a ready signal is given and five seconds later the curtain is opened, giving the animal access to the drawer. It eventually learns to come to the grille when the curtain opens, waits there in a listening attitude until the tone occurs, then promptly opens the drawer and takes out a piece of food. The tone occurs either eight seconds or sixteen seconds after the curtain opens, the delay interval being changed in an unpredictable order. If the animal responds before the onset of tone, the drawer is empty. It therefore learns to inhibit response during the delay period (pp. 263–264).

It will be seen that the training involved learning to respond to the drawer for food, then learning to inhibit that response until the tone was heard. According to Pavlov this would involve both extinctive inhibition and the inhibition of delay, both of which are aspects of internal inhibition. Furthermore, we should expect an irradiation of internal inhibition during the period of delay which should result in observable muscular relaxation and a decrement in other responses. On the

contrary, Wendt found that his subjects, if anything, exhibited greater activity during this period. *In no case did he find relaxation, sleep, or any general decrement in other responses.*[4] He did find a change in activity, which continued to vary during training, but the change always involved some more or less specific response appropriate to the situation and to the species involved. The important consideration is that the monkeys learned to inhibit immediate response to the drawer by *substituting* some other activity. He holds that this substitute activity must be regarded as a competing response which has become dominant over the anticipatory conditioned response, and thus that extinction and delay are explainable in terms of the principle of reciprocal innervation.

Now, how shall these disparate results be reconciled? Both Pavlov and Wendt worked under well-controlled conditions. There was, however, a major difference in their experimental techniques. Pavlov's animals were held stationary in harness while Wendt's were free in a cage. As Wendt has already pointed out, the characteristic behavior of a confined animal in a controlled environment is quiescence after a short period of adjustment, while the behavior of a monkey free in a cage results in continuous self-stimulation from proprioceptive and tactual end organs. Wendt has

[4] It should, perhaps, be stated that Hull (13) has reported some evidence of irradiation of inhibition to another conditioned response. Trace conditioned galvanic responses were established in human adult subjects and during the period of delay between the presentation of the conditioned stimulus and the onset of the conditioned response, the stimulus for a previously conditioned lid closure was given. According to Pavlovian theory, a decrement in amplitude of lid closure would have been expected. On the contrary, results from ten subjects showed little or no indication of a weakening of the conditioned lid reaction during the period of delay of the trace conditioned response. However, the mean latency of the lid responses was found to have increased 17%. Dr. Eliot H. Rodnick (21) informs the writer that he has repeated Hull's experiment under slightly different conditions and has attained a 19% decrement in amplitude and a 15% increment in latency.

Under the conditions of the experiments the response decrement and latency increment can hardly be explained in terms of muscular relaxation. However, it is not necessary to revert to 'irradiation of internal inhibition' for an explanation. One needs only to refer to Wendt's application of the principle of competition between reaction systems. If this explanation is valid, the 'set' to perform some other response should inhibit the interpolated lid response just as much as did the conditioned delay stimulus which Rodnick used.

suggested that perhaps something in Pavlov's experiments facilitated the pattern of relaxation and sleep. He notes three factors (1) restraint of the animal, (2) absence of varied stimulation with its possibility of varied reactions, and (3) repetitive monotonous stimulation. Actually, the first factor is the important one for, in controlled experiments on extinction, stimuli with the exception of those of proprioceptive and tactual origin are eliminated and the presentation of the conditioned stimulus by design is repetitive and therefore monotonous since other stimuli are absent. The point is that in Wendt's experiment muscular relaxation never appeared because the stage was not set for it, while in Pavlov's experiments the stage was set for muscular quiescence, and therefore for relaxation.

It has been experimentally demonstrated, then, that experimental extinction, trace, and delayed responses can be established with little or no indications of irradiation of inhibition and the predictions may be regarded as substantiated. The question immediately arises, how are these phenomena to be explained if not by internal inhibition? Razran (19) has suggested an interpretation in terms of changes in dominance of certain neural patterns. Wendt's interpretation is not essentially different. He believes that his subjects succeeded in inhibiting the anticipatory response to the food drawer when some substitute response apparently had become dominant. Regardless of how that shift in dominance occurred, it offers an opportunity to explain tentatively the inhibition of a given response in terms of the principle of reciprocal innervation of a competing reaction system.

Wendt needlessly belabors his interpretation, however, when he claims that relaxation and sleep also are to be interpreted as competing reactions. It is not intended to suggest that this is never possible. There are many indications for believing that these patterns *may be* conditioned to certain stimuli and some of the instances described by Pavlov should probably be so interpreted. Nevertheless, there are at least three reasons for questioning any generalized conception of relaxation and sleep as competing reactions.

In the first place, when one thinks of a competing reaction, he thinks of a positive muscular reaction. Wendt indicated his realization of this implication when he cited McDougall's references to contracture of the eyelids and to certain postures favoring sleep, as possible muscular components of the competing reaction of sleep. Relaxation however is, to the best of our knowledge, the fundamental prerequisite of sleep, and it is a passive muscular reaction. So far as is known, it is not dependent upon peripheral innervation but instead upon reduction of that innervation. Even if there is a sleep center, its mechanism will probably be found to be a reduction of stimulation at the nuclei controlling muscular tension.

In the second place, even though it is admitted that sleep may become conditioned to normally distracting stimuli, in the usual analysis relaxation and sleep, except in the greatly fatigued or intoxicated organism, is rather a fragile process during its onset. Instead of it competing with other reactions, the latter typically compete successfully with it.[5]

In the third place, if we agree on the ground of passivity and fragility that the competitive potency of relaxation and sleep is questionable, it seems theoretically premature to cast these responses in the mold of other more dominant reaction patterns and speak of only one form of inhibition. Since we know that experimentally induced increments in muscular tension will facilitate response, and conversely that experimentally induced decrements in tension will inhibit response, we have both logical and parsimonious reasons for recognizing an *inhibition due to reduction of proprioceptive facilitation as well as an inhibition due to a competing reaction.*

Summarizing the foregoing discussion several established principles and one postulate appear.

Principle 1.—Decrement in muscular tension usually will be accompanied by a general decrement in response.

[5] It is pertinent here to cite from Pavlov (18, p. 50): ". . . to obtain a smooth curve of extinction of a conditioned reflex it is necessary to maintain the unreinforced conditioned stimulus rigidly constant in character and strength; the environing experimental conditions also must remain absolutely constant . . . the effect of an extra stimulus of even small intensity is a temporary weakening, not of the reflex, but of the progress of experimental extinction."

Principle 2.—Increment in muscular tension usually will be accompanied by a general increment in response.

Principle 3.—The inhibition of a conditioned response may occur without the accompaniment of muscular relaxation and therefore without the phenomenon of general response decrement.

Principle 4.—When muscular relaxation accompanies the inhibition of a conditioned response, decrement in response will be generalized, and the total process must be regarded as two-fold.

Postulate 1.—There are at least two forms of inhibition: (*a*) Reduction of proprioceptive facilitation and (*b*) a shift in dominance to a competing reaction.

These premises may be utilized in a further analysis of behavior.

A. (From 3 and 4.) Muscular relaxation and its accompanying generalized decrement in response is not necessarily related to the inhibition of a conditioned response and when it occurs it must be regarded as an experimental artifact as far as the conditioning process itself is concerned.

B. (From 1 and 2.) During the development of inhibition of a conditioned response which is being accompanied by muscular relaxation, any extraneous stimulus producing an increment in muscular tension will be followed by an increment in the conditioned response.

This prediction has already been realized in the data which Pavlov refers to as 'disinhibition' (*cf.* footnote 5, p. 307).

C. (From Principle 4 and Deductions *A* and *B*.) Disinhibition is not necessarily a law of the conditioned response per se.

It is only known to operate upon that phase of inhibition which has been shown to be an experimental artifact. Disinhibition, therefore, is a law of response as influenced by muscular tension. Whether it also is a law of the pure conditioned response remains to be determined.

D. (From Principles 1, 2, and 4.) After the extinction of a conditioned response which has been accompanied by muscular relaxation, a reinstatement of a more normal level of muscular tension will be accompanied by at least a partial reinstatement of the conditioned response.

This prediction also has been realized in the findings of Pavlov

and others. It is referred to as 'spontaneous recovery' and is supposed to occur within a short time (varying according to the 'depth of inhibition') after the extinction process. In terms of the present interpretation the length of the time interval becomes unimportant. Importance attaches instead to the changes in tension of the skeletal musculature during the interval.

E. (As in *C* above.) Spontaneous recovery is not necessarily a law of the conditioned response *per se.*

Like disinhibition, it is only known to operate upon that phase of inhibition relating to muscular relaxation. It also is a law of behavior as relating to muscular tension, and its operation in the pure conditioned response remains to be determined.[6]

Other deductions relate to the effect of drugs upon the conditioned response.

F. (From Principle 1.) The extinction of a conditioned response should be facilitated by administration of a drug known to effect muscular relaxation.

G. (From Principle 1.) The extinction of a conditioned response should be impaired by administration of a drug known to interfere with muscular relaxation.

[6] Humphrey (14) has reported disinhibition (which he terms 'dehabituation') and spontaneous recovery in experiments on negative adaptation in controlled environments. If, as has been postulated, these phenomena are the result of tension increment, it should be possible to find that tension decrement had occurred previously. Unfortunately, Humphrey's data do not provide the necessary information. The literature was searched and paradoxically the only pertinent data were found in Pavlov (18) where the 'investigatory reflex' of a dog to a whistle was negatively adapted. Pavlov says (p. 255) "Special experiments . . . showed that the disappearance of the investigatory reflex is based on the development of inhibition, and is in all details analagous to the extinction of conditioned reflexes." Even more specifically (p. 256), "The inhibition of the investigatory reflex invariably leads to drowsiness and sleep (even more easily than the inhibition of conditioned reflexes)."

It is thus found that under certain conditions negative adaptation results in relaxation, which, among other observations, seems to have led Pavlov to state that it also is mediated through an irradiation of internal inhibition. It is known, however, in common experience, that adaptation of a response may occur in active and uncontrolled environments. Therefore it seems that negative adaptation, like extinction, must be considered as a twofold process when it occurs in a controlled environment, and that the relaxation which there is observed must likewise be regarded as essentially artificial to adaptation as such. Perhaps both 'dehabituation' and 'disinhibition' as well as 'spontaneous recovery' might more accurately be described as 'de-relaxation.'

Switzer's work (22) concerning the effect of caffeine on extinction already affords support of the latter deduction.

Another deduction takes the form of an explanation of the inhibition which is purported to adhere to the conditioning process itself. It has been reported by Platonov (*cf.* Razran's review, 20) and more recently by Hovland (12), that a conditioned response immediately after the conditioning process is occasionally lesser in magnitude than it is some time later. Since it seems that monotony of stimulation is one factor which effects muscular relaxation, and since there is a certain monotony in the conditioning process especially if the stimulations occur at short intervals, it is possible to assume that some relaxation of the skeletal musculature may occur during the conditioning process itself. The logical deduction follows:

H. (From Principle 1.) If, during the conditioning process, there occurs a decrement in muscular tension there will occur a decrement in the conditioned response.

If this deduction is the proper explanation for the inhibition in question then the conditioned response so inhibited should be subject both to disinhibition and spontaneous recovery. Only the latter has yet been demonstrated.

A final inference would question the validity of a smooth curve of experimental extinction. It proceeds from the assumptions that (*a*) learning is not a smoothly progressive process but is interrupted by numerous plateaus and regressions, and that (*b*) muscular relaxation, if uninterrupted, is a smoothly progressive process. A slight degree of relaxation reduces the postural substrate. The facilitative effect upon higher nervous centers is thereby reduced and cortical and bulbar stimulation of the peripheral musculature are in turn decreased, again diminishing the proprioceptive bombardment upon the higher centers. The latter assumption would, of course, explain parsimoniously the smooth curve of extinction which Pavlov and others have reported, but leave the true nature of the curve unpredicted. If Wendt is right in maintaining that extinction of one response involves learning

of another, the curve of pure extinction may be the inverse of the learning curve of the newly dominating response.

It is apparent that the picture of inhibition presented in this paper is radically different from that presented by Pavlov. If the present analysis is tenable, much of the work of Pavlov and his pupils is questionable, for their researches on inhibition were accompanied to such an extent by drowsiness and sleep that Pavlov, at least, regarded the whole as one process. Probably most of the data concerning inhibition present an erroneous picture *as far as the conditioning or learning process itself is concerned.* In short, the work on inhibition of conditioned responses must be repeated under conditions of known stable muscular tension before many laws of conditioned responses can be recognized and applied to the more complex processes of human behavior.

REFERENCES

1. Bagchi, B. K., An electromyographic study with respect to speed of movement and latency, disparate and reciprocal innervation, attention and relaxation, *Psychol. Monog.* (to be published).
2. Bekhterev, V. M., General principles of human reflexology, New York: International Publishers, 1932, pp. 467.
3. Beritoff, J. S., On the fundamental nervous processes in the cortex of the cerebral hemispheres, *Brain*, 1924, 47, 109–148.
4. Bills, A. G., The influence of muscular tension on the efficiency of mental work, *Amer. J. Psychol.*, 1927, 38, 227–251.
5. Chappell, M. N., Inhibition, facilitation, learning, Psychol. Rev., 1931, 38, 317–331.
6. Dodge, R., Conditions and consequences of human variability, New Haven, Conn.: Yale University Press, 1931, pp. xi, 162.
7. Freeman, G. L., Mental activity and the muscular processes, Psychol. Rev., 1931, 38, 428–449.
8. ——, The spread of neuromuscular activity during mental work, *J. Gen. Psychol.*, 1931, 5, 479–494.
9. ——, Studies in muscular tension [Collected papers], 1929–1933, pp. 182.
10. ——, Physiological psychology, New York: Ronald Press, 1934, pp. 579 + xv.
11. Guthrie, E. R., The psychology of learning, New York: Harper & Bros., 1935, pp. viii, 258.
12. Hovland, C. I., The effects of varying amounts of reinforcement upon the generalization of conditioned responses, *Psychol. Bull.*, 1935, 32, 731–732.
13. Hull, C. L., The alleged inhibition of delay in trace conditioned reactions, *Psychol. Bull.*, 1934, 31, 716–717.
14. Humphrey, G., The nature of learning in its relation to the living system, New York: Harcourt Brace, 1933, pp. vii, 296.

15. JACOBSON, E. AND CARLSON, A., The influence of relaxation on the knee jerk, *Amer. J. Psychol.*, 1925, 73, 324–328.
16. JACOBSON, E., Progressive relaxation: a physiological and clinical investigation of muscular states and their significance in psychology and medical practice, Chicago: University of Chicago Press [C. 1929], pp. xiii, 428.
17. LASHLEY, K. S., Nervous mechanisms in learning, in 'The Foundations of Experimental Psychology' (Ed. by Carl Murchison), Worcester: Clark University Press, 1929, pp. 907.
18. PAVLOV, I. P., Conditioned reflexes: an investigation of the physiological activity of the cerebral cortex, Oxford University Press: Humphrey Milford, 1927, pp. 430–xv.
19. RAZRAN, H. S., Theory of conditioning and of related phenomena, PSYCHOL. REV., 1930, 37, 25–43.
20. ——, Conditioned withdrawal responses with shock as the conditioning stimulus in adult human subjects, *Psychol. Bull.*, 1934, 31, 111–143.
21. RODNICK, E. H., Characteristics of delayed and trace conditioned responses (to be published).
22. SWITZER, ST. CLAIR, The effect of caffeine on experimental extinction of conditioned reactions, *J. Gen. Psychol.*, 1935, 12, 78–94.
23. TUTTLE, W. W., The effect of attention or mental activity on the patellar tendon reflex, *J. Exper. Psychol.*, 1924, 7, 401–419.
24. WENDT, G. R., An interpretation of inhibition of conditioned reflexes as competition between reaction systems, PSYCHOL. REV., 1936, 43, 258–281.
25. WENGER, M. A., External inhibition and disinhibition produced by duplicate stimuli, *Amer. J. Psychol.*, 1936, 48, 446–456.
26. ——, An investigation of conditioned responses in human infants, in 'Studies in Infant Behavior,' III, *Univ. Iowa Stud., Stud. in Child Welfare*, 1936, 12, No. 1, pp. 207 (pp. 7–90).
27. —— AND IRWIN, O. C., Fluctuations in skin resistance of infants and adults and their relation to muscular processes, in 'Studies in Infant Behavior,' III, *Univ. Iowa Stud., Stud. in Child Welfare*, 1936, 12, No. 1, pp. 207 (pp. 141–179).
28. WINSOR, A. L., Observations on the nature and mechanism of secretory inhibition, PSYCHOL. REV., 1930, 37, 399–411.

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IS CHEERFULNESS–DEPRESSION A GENERAL TEMPERAMENTAL TRAIT?

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Early in 1935 we carried out a series of three interrelated experiments upon affective processes. Although each experiment had its independent aim and method, all three bear directly upon a common characteristic of the individual: the tendency to respond in a cheerful, pleasing manner or the reverse. In one of the experiments the subjects were required to smell a series of fourteen odors and to report whether they liked or disliked them. In another, the experimental task was to recall pleasing and displeasing words under conditions which made it equally possible to recall either kind. In the third experiment the subjects rated their mood for the previous day on a seven-point scale extending from 'highly elated' to 'extremely gloomy.' [1]

If we accept the common assumption that cheerfulness-depression is a general trait of the individual, we would expect to find positive correlations among the scores on the three tests. The prevailing view is that a cheerful temperament reveals itself in many ways—by smiling, being readily pleased by things and people, by pleasing words, pleasing actions, etc. According to this view the subjects who are most readily pleased by odors ought also to be the ones most inclined to recall pleasant rather than unpleasant words. Further, the generally cheerful individuals on the average should rate their moods as relatively cheerful. If there is in fact a *general* trait of cheerfulness-depression, a correlational study

[1] These experiments have been described in the following papers and the reader is referred to them for details: (1) A group experiment upon the affective reaction to odors, *Amer. J. Psychol.*, 1937, **49**, 277–286; (2) A study upon the recall of pleasant and unpleasant words, *Amer. J. Psychol.* (to appear); (3) Laughing and weeping, cheerfulness and depression: a study of moods among college students, *J. Soc. Psychol.* (to appear).

of our experimental results should reveal positive coefficients throughout.

In the following paragraphs we will first present the results of a correlational study and then interpret them with respect to the question at issue: the generality of the temperamental trait cheerfulness-depression.

Results

1a. *Rank order correlation between affective reaction score to odors and mood rating.*—In the experiment with odors a group of fifty-six subjects smelled a standard series of odors and reported immediately for each one "I like it" or "I dislike it." There were fourteen presentations in the series, including thirteen odors and one odorless control; the order of presentation was varied from day to day to prevent memorizing. On the basis of his affective reactions each subject was given a rating which indicated the degree to which he was pleased by the standard series of odors.

The ratings on the affective reaction test have been found to be highly consistent from day to day and from group to group; for this reason we believe them to be dependable. A rank-order correlation between the preferential ratings of the present group of fifty-six subjects and an equivalent group of one hundred college students gave a positive coefficient of 0.93. Other correlations between groups of children and college students gave coefficients of similar magnitude.[2]

In addition to smelling the odors every subject, on the same experimental day, rated his average daily mood. A seven-point scale was employed, as follows:

Highly elated	Moderately cheerful	Mildly cheerful	Indifferent	Mildly depressed	Moderately depressed	Extremely gloomy

[2] In a previous study product-moment coefficients of correlation were computed among three age groups: I, ages 7–9; II, ages 10–13; III, ages 18–24. The values obtained for r were: between Groups I and II, 0.93; between Groups I and III, 0.91; between Groups II and III, 0.96. The work is described in E. H. Kniep, W. L. Morgan and P. T. Young, The relation between age and affective reaction to odors *Amer. J. Psychol.*, 1931, 43, 414f.

Assuming a general temperamental factor of cheerfulness-depression, we should expect, as noted above, that cheerful individuals would be inclined to rate their moods as cheerful, and also that they would tend to be readily pleased by the odors. To make a test of the expected relationship we arranged the subjects in two rank orders: first, the subjects were ranked from the one who was most readily pleased by the odors to the one who was the least pleased by them; second, the subjects were ranked from the one who rated himself the most cheerful to the one who rated himself the most depressed. In the latter case we limited our analysis to subjects who had been present on four or more of the experimental days; this cut the number down to thirty-four. For these thirty-four subjects a rank-order coefficient of correlation was computed. This coefficient between the tendency to rate the odors as pleasing and the tendency to rate one's mood as cheerful turned out to be minus 0.02 (P.E.$_r$ = 0.12) which means, of course, that there is no relation between the two variables.

We were not fully convinced by this result and so made another attack upon the problem, which is described below.

1b. *Product-moment correlation between affective reaction score to odor and mood rating.*—We have, all told, 306 records of daily mood ratings matched by an equal number of affective reaction scores to odors. Both kinds of measure were obtained from the same subjects during the same experimental hours. We prepared a scattergram in which mood rating was plotted against affective reaction score, and from this computed a coefficient of correlation by the product-moment method.

The value of r turned out to be minus 0.29 (P.E.$_r$ = 0.03; r/P.E.$_r$ = 8.3). This value, though low, is significant; but it is *minus*. It means that so far as the variables are related, persons who rate their moods as cheerful are to a slight extent inclined to *dislike* the odors. This finding is directly counter to expectation on the assumption of the general temperamental factor.

The soundest conclusion in view of the low degree of relationship is that the two variables—tendency to rate one's

mood as cheerful and tendency to rate odors as pleasing—are not positively correlated. There is thus no evidence in these results for a general temperamental factor.[3]

2. *Relation between affective reaction to odors and inclination to recall pleasing words.*—In addition to reporting his likes and dislikes for odors, and evaluating the average level of his daily mood, every subject was required during the same experimental hour to write down pleasing and displeasing words. The pleasing words were listed in one column and displeasing in another, but conditions were so arranged that it was equally possible to write down either pleasing or displeasing words. We assumed that the ratio between the number of recalled pleasant and unpleasant words would indicate the general level of cheerfulness-depression of the subjects.

The P/U ratios were moderately constant throughout. For the group as a whole the average values varied, over a period of thirteen days distributed through three weeks, between 54.0 and 58.7 per cent of pleasant words.

In working out a coefficient of correlation a scattergram was prepared by plotting affective reaction score to odors against percentage of pleasant words recalled. Owing to the fact that the word and odor tests were not both given on every experimental day, we have only 266 usable records obtained during eight days from a total of fifty-two subjects. The number of cases, however, is ample for our purposes.

A product-moment coefficient of correlation between the tendency to report odors as pleasing and the tendency to recall pleasing words turned out to be minus 0.007 ($P.E._r = 0.04$; $r/P.E._r = 0.17$). Once again we found a negative, really a zero value. The proneness to think of pleasing words and the

[3] In a previous experiment a single subject was discovered who on one of twelve experimental days described a distinctly unpleasant mood; on this day her affective reaction score to odors made a decided drop relative to days with normal mood. It is, of course, dangerous to generalize from one striking instance. It should be pointed out that the mood rating in this case was for *present* mood whereas in the experiment described in the text the mood rating was for the previous twenty-four hours. An experiment is indicated in which *present* mood is varied just prior to smelling the odors, to determine the effect of such variation upon affective reactions. See *Amer. J. Psychol.*, 1930, 42, 26.

tendency to be pleased by odors have no demonstrable relationship to each other. If there were a general temperamental factor of cheerfulness, there should be a positive correlation.

3. *Correlation between the tendency to think of pleasing words and the tendency to rate one's mood as cheerful.*—To complete the picture we prepared another scattergram, plotting the percentage of recalled pleasant words along one axis and mood rating along the other. There were 261 available pairs of data for this scattergram.

The mood ratings have a reasonable degree of self-consistency on the part of individual subjects. In terms of the seven point mood-rating scale the average deviations range from 0.00 to 1.50 with the most frequent average deviations between 0.61 and 0.90 scale points.

The product-moment coefficient of correlation was found to be plus 0.29 ($P.E._r = 0.04$; $r/P.E._r = 7.5$). The value of r, though low, is significant. In view of the zero and negative correlations reported in the above paragraphs we must proceed with caution to the interpretation of this one positive result.

Interpretation

The first thing to notice in interpreting these results is that the tendency to be pleased by odors has no positive relation to the tendency to think of pleasing words. The lack of positive correlation may be explained by the fact that the primary affective response to olfactory stimulation is automatic, perhaps reflexive; it is determined by the stimulation of olfactory cells, and the excitation of neurons in the olfactory bulb, hippocampus and thalamus. Details of the bodily mechanism which determines the affective response to odors are unknown to the writer, but it is certain that the mechanism is anatomically distinct from that brought into play by the instruction "write down pleasant and unpleasant words." Recalling words is a process which brings the cerebral speech mechanisms into function, whereas liking or disliking odors is a basic physiological process of a wholly different kind.

This result strengthens a conclusion of Darrow [4] that there are "two physiologically as well as psychologically distinguishable processes which have frequently been designated as 'emotion,' namely the immediate reflex response to sensory excitation, and the response mediated by associative processes or ideas aroused by the stimulus." Darrow demonstrated that the two processes are distinct on the basis of blood pressure records and the galvanic skin-reflex.

Next, let us reflect upon the finding that the tendency to be pleased by odors is wholly unrelated to the degree of cheerfulness as determined by self-ratings of moods. In the study upon moods [5] one of the outstanding results is that moods of cheerfulness and depression are dependent primarily upon meaningful events within the *social* environment. From this fact it may be inferred that the situations and events which change one's mood bring into function the complex processes of the cerebrum. The neural mechanisms which regulate affective reactions to odors are anatomically distinct from those which regulate moods, and this fact offers a satisfactory basis for explaining the zero and negative correlations which we found.

The positive correlation which we obtained between the tendency to recall pleasant words and the tendency to rate one's mood as cheerful is so low that it is of little practical importance. There may, however, be some degree of overlapping in the neural processes of the two activities. The experimental tasks were alike in that both demanded from the subject that he recall something pleasant or unpleasant.[6]

Whether or not there be a common factor underlying the activities of mood rating and verbal recall of pleasant and unpleasant words, the relationship between each of these activities and that of making affective response to odors is

[4] C. W. Darrow, Electrical and circulatory responses to brief sensory and ideational stimuli, *J. Exper. Psychol.*, 1929, 12, 267–300.

[5] See foot-note 1, p. 313.

[6] In rating his mood the subject had to recall the pleasing and displeasing situations of the previous day; in one part of the mood-rating blank he was specifically instructed to list events which left him more cheerful or more depressed.

negative or zero. We conclude that the temperamental trait of cheerfulness-depression is not wholly general.

If the above interpretations are sound, cheerfulness-depression is restricted to a particular sphere. This trait is revealed in the sphere of *social* behavior. The common manifestations of cheerfulness and depression which lead to the remark "A is a cheerful person" are certainly found in *social* situations. What reason is there to expect cheerfulness-depression to be shown by the affective responses to olfactory stimulations?

The findings of this study raise a fundamental question as to how general and how specific the trait of cheerfulness-depression really is within the personality. A research upon this problem is now under way in the writer's laboratory.

Summary

A test in which the subjects reported liking and disliking for odors gave results with zero or negative correlation between two other affective tests: (*a*) the recall of pleasing and displeasing words, and (*b*) the rating of one's mood for the previous day as cheerful-depressed. From the failure to find positive correlations the conclusion is drawn that cheerfulness-depression is not a general trait of the personality. The suggestion is made that cheerfulness-depression is revealed primarily by the responses an individual makes to his social environment. These responses, physiologically considered, are upon the cerebral level. The responses to simple olfactory stimulation probably occur at a lower level—that of the hippocampus or thalamus.

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REFLEXOLOGY AND THE PSYCHOBIOLOGICAL APPROACH

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The importance of the conditional response movement for the science of behavior is so evident that there is no need to elaborate the point. From the standpoint of methodology alone, it must unquestionably rank as one of the major contributions to the analysis of behavior. It has brought into the laboratory many complex and difficult problems and it will unquestionably be a prolific source of future contributions. As a formal school of thought it has been of tremendous influence. It is beyond the scope of this paper, however, to attempt to evaluate conditioning as a total movement or to assess its ultimate significance. The objective of the present discussion is to attempt an appraisal of the systematization of behavior elaborated by the Pavlovian school. It is obvious that conditioning can thus be discussed without in any way calling into question its unassailable position of importance as a methodology. There are several cogent reasons for undertaking such a survey. The Pavlovian systematization of behavior is, perhaps, one of the most explicit, and one of the most inclusive formulations ever attempted. It has been elaborated directly from experimental work. Inasmuch as the underlying studies represent one of the major interests of this laboratory over a period of two decades or more, they, and the deductions to which they have lead, merit the most careful consideration.

Within the past decade there has been an increasing tendency for psychologists—particularly those in the field of learning—and psychiatrists to take over the terminology of the Pavlovian system. Since the danger of reification is now

*This paper was begun while the writer was associated with the Phipps Clinic of the Johns Hopkins Hospital.

so generally recognized, it is reasonable to assume that authors who use terms such as 'internal inhibition,' 'external inhibition,' 'irradiation,' and 'concentration' find the traditional terminology inadequate to designate the phenomena under consideration. And since Pavlov adopted these terms to convey highly specific connotations, subsequent writers are presumably bound to employ them in essential conformity with their original significance.

It has been pointed out in a previous paper (9) that the Pavlovian formulation of behavior, as a formal system, rests upon one basic physiological concept. This is the principle of irradiation or the tendency of cortical disturbances, *i.e.*, excitations and inhibitions, to spread from a delimited origin to the adjacent surrounding areas. (The complementary aspect of irradiation is the tendency for disturbances which have spread over an area of the cortex to concentrate about their origin.) As a concept, the spreading of a cortical excitation has a perfectly sound basis in experiments involving electrical stimulation of the cerebrum. And it is clear that Pavlov's general principle of irradiation had its prototype in such phenomena, as shown by the following quotation.

> In 1870 the experiments of Fritsch and Hitzig laid the foundation for an exact and successful physiology of the brain, and physiologists made acquaintance with the important and not sufficiently appreciated fact that the stimulation of a certain point of the hemispheres always tends to irradiate quickly: the initial contraction of a definite group of muscles under the influence of a continuous or strong stimulus passes over into tonic convulsions of the whole body.
>
> It is a well known fact, which may be observed with all agents, that when they have just become conditioned stimuli their action, during the first days, is very general, *i.e.*, there now act as conditioned stimuli (calling out the same effect as the original stimulus) all phenomena similar or akin to the elaborated stimulus. Only by and by, under the influence of definite circumstances, does the conditioned stimulus become specialised (specific), *i.e.*, the action of these accessory stimuli, which do not coincide with the conditioned reflex, is obliterated by the process of inhibition, and only the conditioned stimulus remains active. Now it is quite natural to conceive the effect as a phenomenon due to irradiation.

These considerations and our additional data give us the right to accept the law of irradiation and concentration for impulses arriving in the hemispheres; first these excitations spread, and flood the whole cerebrum, and later they collect in certain, definite points. (12, pp. 208–209. See also pp. 217–218 and Chapter XXI.)

It is evident from the above quotation that the Pavlovian irradiation principle was derived by analogy to epileptoid disturbances produced when the brain is stimulated with strong electrical shocks. When an electrical stimulus is of such magnitude that it induces an epileptoid attack ending in generalized convulsions, the underlying physiological process is obviously pathological. And any attempt to explain the normal activity of the nervous system upon such a basis is extremely questionable. It should also be noted that the rate at which successive muscles are activated in such an epileptoid attack involves time units which are of an entirely different category than those characterizing the neural activity underlying the majority of psychobiological reactions. The relatively slow propagation of this abnormal disturbance and its sweeping inclusiveness constitute qualitative and quantitative evidence which would strongly oppose any argument for a significant similarity between this and normal neural activity. The diffuseness of this type of propagation is fundamentally opposed to that characterizing conduction along discrete pathways. It may be questioned whether these epileptoid phenomena can be referred exclusively to the cortex or whether lower centers may not play an important rôle (See Starling's 'Human Physiology,' 6th edition, page 292). This point, however, is one to be established by experimental investigation and is only raised here to emphasize the fact that very little is known regarding the intimate nature of such abnormal disturbances. Denny-Brown (3) has pointed out numerous additional objections to Pavlov's irradiation hypothesis, based upon neurophysiological considerations, but limitations of space preclude a review of his stimulating article.

If the Pavlovian systematization of behavior is to be understood it is essential that the principle of irradiation be clearly grasped. Pavlov gives the following lucid discussion

in connection with a study of the irradiation of an inhibition. "Our explanation is purely physiological, purely objective, purely spatial. It is obvious that in our case the skin is a projection of the brain mass. The different points of the skin are a projection of the points of the brain. When at a certain point of the brain, through the corresponding skin area on the shoulder, I evoke a definite nervous process, then it does not remain there, but makes a considerable excursion. It first irradiates over the brain mass, and then returns, concentrating at its point of origin. Both of these movements naturally require time. Having produced inhibition at the point of the brain corresponding to the shoulder, when I stimulated another place (the thigh) I found that inhibition had not yet spread this far. After twenty seconds it had gotten here; and in twenty seconds, though not before, complete inhibition occurred at this point. The concentration requires forty seconds, and after sixty seconds from the end of the zero irritation on the shoulder, we already had a complete restoration of the reflex, on the second spot (the thigh). But on the primary place (the shoulder) the reflex was not yet restored even after five to ten or fifteen minutes. . . . If actually we have a movement, then consequently in all the intervening points we should be able to predict the effect, judging by the fact that this movement occurs in two directions. I take only one intermediate point. . . . In proportion to its proximity to that area where I produce the inhibition it will be inhibited. Consequently in it the zero effect appears sooner and lasts longer—while the inhibition passes further and then recedes. At this spot the return to the normal irritability occurs later . . ." (**12**, pp. 273–274).

It is clear from the above statement that the essence of the Pavlovian irradiation principle is that of a gradually spreading disturbance. And it is of interest to discuss it from this fundamental viewpoint without further qualification. The most obvious way to demonstrate that a disturbance spreads from one point or one region to another is to note the respective instants at which it passes two consecutive points along its path. This holds true whether one is working with

the propagation of mechanical disturbances through liquids or with bioelectrical changes through tissue. The conditioning experiments which are used as evidence for irradiation or spread of a cortical disturbance are designed along just such lines. In the instance of an epileptoid attack evoked by a strong electrical stimulus, one could measure the respective times at which major muscle groups were brought into play, in, for example, hundredths of a second, whereas if one were studying the propagation of sound through water one would have to use much smaller units.

It is obvious from the time units which have been chosen in observing the *propagations* of the cortical disturbances that the process of irradiation was regarded as constituting a relatively deliberate movement. For example, in Podkopaev's study (9) the shortest time interval tabulated is 45 seconds and the greater part of the study was made with a minimal interval of 4 minutes. In other words, this experiment, by its inherent design, is inadequate to demonstrate the nature of that vast category of neural processes which are encompassed by a time limit of 45 seconds. And it should be noted that in those experiments where a measure of secretion was made at zero seconds after an inhibitory stimulus, the measuring of the secretion took thirty seconds. Suppose, however, that we assume ten-second measurements, and disregard the time consumed in the propagation of the inhibitory effect. From the psychobiological standpoint, ten seconds may be a relatively long period of time. A man can type approximately 100 characters or run 100 yards in that interval. And it is self evident that such a vast category of neurophysiological and psychobiological events can take place within that span of time that a formulation or system elaborated upon ten-second data could not in any sense be considered as comprehensive or reasonably precise.

One aspect of the theory of irradiation which has doubtless been the cause of much misunderstanding and misinterpretation involves the concepts of gradient and irradiation. These two terms stand for fundamentally distinct phenomena and it is of the utmost importance that they not be confused if one is

to evaluate the Pavlovian theories. When one measures the rate of metabolism of nervous tissue in a developing embryo, it is found that there is a gradient of activity from the oral to the caudal extremities. Such an observation in no way demonstrates that the gradient is due to an irradiation of the energy of chemical change from the anterior to the posterior end. The energy gradient, here, is a product of the characteristics of the cellular activity in the respective regions. A mountain is tangible evidence of a gradient but a surveyor cannot assume irradiation whenever he measures a grade. On the other hand, the spreading of combustion through a sheet of gunpowder would represent irradiation but need not be characterized by a decrement in speed or intensity of chemical change. It must be apparent, therefore, that there is no justification for assuming irradiation from the gradient phenomena or vice versa, even though both phenomena are sometimes found in association as in the case of the diffusion of heat through a liquid medium.

In a previous paper (**9**), the present writer reviewed the data of six conditioning experiments carried on in Pavlov's laboratory, which were designed to demonstrate irradiation. Three of the studies were presented in the chapter on irradiation in Pavlov's book, 'Conditioned Reflexes,' as evidence for the spatial spreading and concentrating of cortical disturbances. When the data of the original studies were statistically tabulated and reduced to graphs it was clear that without exception each study conclusively demonstrated that when, at a particular time, stimulation of one point showed a decrease in the salivary response, all points tested on that dog registered a synchronous decrease. The fact that the fluctuations were so clearly and unmistakably synchronous for all spots tested, in spite of the great degree of variability of the data in these studies, is most significant. Whatever the explanation of these phenomena, the data positively and conclusively demonstrate the absence of the slowly spreading type of cerebral disturbance essential to substantiate the Pavlovian principle of irradiation. If it is found that a point at ten units of distance from the origin of the disturbance

registers its passing synchronously with a point at one unit of distance, "an actual progression or regression of the inhibitory effect can not be demonstrated." All six studies are completely consistent in that they fail to demonstrate the basic principle of spread or irradiation.

The data of one of the six studies, namely that of Anrep, which as was previously pointed out (9) are fundamentally a-typical as regards any other study ever reported from Pavlov's laboratory, differ from the other five studies which were reviewed, in exhibiting a gradient. Nevertheless, Anrep specifically states that his data do not actually demonstrate the spreading of the wave of inhibition; he arbitrarily assumes that the irradiation must have taken place too rapidly for him to measure. Although these data are, by inspection, clearly a-typical of conditional salivary reactions in general, it is of interest to interpret what they imply—taking them at their face value. Since, as Anrep notes, all spots indicate a synchronous fluctuation during the experiment, the most obvious assumption suggested by the data is that a particular degree of inhibition must be propagated from its origin to the cortical representative of the hindpaw (8 units of distance) at 4 times the rate at which it spreads to the chest or the pelvis (2 units distance), and twice as fast as it spreads to the forepaw (4 units distance). In other words, for the maximal degree of inhibition or any particular percentage of inhibition to reach two different spots representing different distances, the underlying disturbance would have to be propagated at an average rate of speed in proportion to the distance to be traversed. This would constitute an incremental rate of propagation of a most unusual type, for at the same time that the speed of propagation increases with distance, the intensity must decrease in relation to the same factor. This would appear to be a rather remarkable sort of spread; and one is at a loss to find any natural biological prototype. It does demonstrate that if one accepts the fundamental premise of the Pavlovian hypothesis of irradiation and upon this basis attempts to explain these data of Anrep, the premise and the data prove incompatible. Since, as it was pointed out in the

earlier paper (9), there is very good reason to reject the data, the inevitable conclusion as regards the immediate instance is that neither theory nor data are satisfactory.

Since the publication of the present writer's initial review of the experimental data underlying the Pavlovian systematization of behavior (9), a study entitled 'The Irradiation of a Tactile Conditioned Reflex in Man' has been presented by Bass and Hull (1). This experiment was carried out at Yale University with the most admirable precautions as to rigid experimental control. There is considerable reason to believe that it is one of the most carefully controlled experiments in this field that has ever been published. Upon the basis of their experimental results, the authors conclude:

> It has been shown experimentally that there is in adult human subjects a definite spread of cutaneous conditioned excitatory tendency from the shoulder toward the feet and from the calf toward the head. There has also been found a definite analogous spread of the inhibition resulting from experimental extinction. In both cases the spread takes the form of a gradient, the magnitude of the tendency diminishing progressively with distance from the point of origin. In this respect the present results are in complete accordance with the findings of Anrep, despite the wide differences between the two investigations in the subjects used, the stimuli employed, and the reactions measured (1, p. 65).

These conclusions, if substantiated, would be most significant. Unfortunately, however, the writers have apparently confused the concept of gradient with that of spread or irradiation. It was pointed out above that while these two phenomena are sometimes found in association it is entirely unjustifiable to assume the one from the other. In Anrep's study, which the authors state they confirm, as well as in all other studies in this field, measurements were taken at a number of successive intervals of time. The data of Bass and Hull are presented as a single composite curve for the irradiation of the excitation, and a similar composite curve for the second part of the study, involving inhibition. Presumably, the authors intend a composite curve to represent the spread of inhibitory effect at a representative time interval separating

the moment of giving the inhibitory stimulus and the testing of its effect at the various points spread out over the body. But a brief consideration of the matter makes it evident that if the phenomena of spread are to be demonstrated one must have a whole series of curves representing measurements at successive intervals of time. For example, to show movement in the planets one must have two successive photographs. One brief exposure by itself is inadequate to trace displacement.

The fact that Bass and Hull fail to mention the time span between the administering of the excitation, or the inhibition, and the measuring of the resulting effect at a distant point strongly suggests that they failed to grasp the essential plan of the classical experiments on irradiation. In the first part of their paper the writers carefully note the duration of the stimuli, but make no mention of the precise interval between the initial reinforcing stimulus and the testing of the spread effect, by stimulating the remaining points. In the Pavlovian papers, in spite of drastic condensation, such data are carefully noted, as they constitute the fundamental plan upon which the experiment is built. The protocol presented by the writers (see their Table I) shows that the first reinforcement involved five reinforcements and the remainder only three. If these data are to be grouped and anything deduced about the spread effect one must assume that the actual propagation of the excitatory disturbance is delayed until the last reinforcement is given. Otherwise, one would be summarizing the effects of staggered propagations. In addition, the method of taking measurements would result in a staggering of tests with respect to the last reinforcing or extinguishing stimulus. If, for example, a spontaneous electrodermal reaction arose following a series of inhibitory or excitatory stimulations at point 0, the experimenter would presumably delay his stimulation of a second spot in the routine measurement of spread.[1] This would delay certain test measurements so that a specific spot, such as number 2, might be tested at the time interval at which spot 3 was scheduled to be measured.

[1] This assumption is deduced from the notation on page 52 of Bass and Hull's paper (1) which states that, "a test stimulus was never given until the galvanometer had come to rest," although it does not explicitly refer to the latter part of the study.

In an introductory footnote, Bass and Hull state, "In the present article the writers use the terms 'spread,' 'irradiation' and 'generalization' as a matter of convenience, but do not thereby subscribe to any neurological theory whatever." The preceding discussion and the writer's earlier paper on irradiation have been developed from three primary assumptions explicit in the Pavlovian hypothesis of spread. The first is that an inhibitory or an excitatory disturbance is propagated through successive regions in consecutive order. Secondly, the disturbance spreads from a delimited region in the brain, corresponding to the point on the skin at which the excitatory or inhibitory disturbance is administered. Thirdly, the rate of spread is relatively slow. The writer believes that Bass and Hull subscribe to these three assumptions. Their acceptance of the first point is evident in their conclusion and in their planning of the experiment. That they accept the third assumption is demonstrated by their choice of an experimental method limited to the measuring of relatively deliberate changes. If a photographer sets the shutter of his camera at one-quarter of a second it is not unreasonable to infer he does not expect to take the picture of a bullet fired from a gun. The term irradiation, as used in this field, apart from its connotations of 'spread' is meaningless. If one uses the term and does not intend any implication of nervous activity one must assume that the spread takes place in some hypothetical stratum, *e.g.*, the 'mind.' In view of Professor Hull's extensive criticisms of the use of improperly defined terms (7), the writer assumes that when Bass and Hull use the term irradiation they would not imply a non-nervous, *e.g.*, symbolic 'spread' phenomenon. In physiology, the term irradiation has a perfectly legitimate meaning and usage. When it is used in the field of conditioning it must inevitably convey connotations of a slowly spreading cortical excitation or inhibition since that is the essence of the Pavlovian hypothesis. It has been pointed out at some length both in the writer's earlier paper on this subject and in the above discussion that the hypothesis is entirely an indirect assumption. Inasmuch as the data completely refute the hypothesis and since the

original concept is based upon an extremely questionable analogy there would no longer seem to be justification for employing the term in connection with the phenomena of conditioning.

To use the term irradiation and to deny, at the same time, that one implies any of the Pavlovian connotations inevitably suggests reification. For one need not invoke the concept of 'spread' to interpret such data as are presented by Bass and Hull. It has been known for some years that a psychobiological organism may react to stimuli on the basis of relative similarity.[2] The problem may be considered from the standpoint of the animal's differentiating between a tactile stimulus on the shoulder which is invariably followed by food and a tactile stimulus on the thorax given without food. Upon sufficient training an animal can learn to discriminate between the two stimuli so that he always gives a salivary secretion to stimulation of the shoulder but gives no secretion to stimulation of the thorax. In one day's training series one may give such an animal a so-called inhibitory stimulus at the thorax and after a brief interval follow it with the excitatory stimulus at the shoulder. In the perfectly trained dog the stimulus at the thorax will not inhibit secretion to a subsequent stimulus at the shoulder. If it were a human subject, and the experimenter were not a reflexologist, he would say, "Of course not. The subject can discriminate between the two stimuli." On the other hand, if the thoracic stimulus elicits anticipatory reaction tendencies which are elements of the response pattern given when a stimulus, contrary to routine, is not followed by food—and implicit as well as explicit reactions must be taken into consideration—a subsequent stimulation of the shoulder would tend to evoke a subnormal quantity of saliva. One common condition predisposing toward such an inhibition would be imperfect learning. The subject might react to a particular aspect of one stimulus which is present in the other, *e.g.*, similar rhythm, in the case of repetitive tactile stimuli. In the Bass and Hull study the recording of the electrodermal response was such that in order to get an adequate magnitude

[2] See Gundlach's important discussion of this subject (4).

of response, the subjects, who were paid for their assistance, were subjected to extremely intense electrical shocks. Such treatment would inevitably arouse a state of tenseness and apprehension which would facilitate the subjects' reacting to any stimulus that was in some aspect similar to the original conditioning stimulus. A priori, there is no particular reason why a dog that is trained to react to one tone might not secrete amounts of saliva to other tones which, in a general way, correlate with the pitch differences between the original and new stimuli. This would possibly be particularly true of the earlier stages of training. Similarly, the spot on the shoulder may, in effect, be more similar to the point on the back than a point on the leg. Guthrie (5) has presented one effective formulation of the basis of such similarities in terms of conditioned contractions in related muscle groups. The terms irradiation and concentration imply an underlying physiological mechanism which somehow explains the psychobiological data of the experiment, and as a consequence they obscure solution of the problem.

It is sometimes asserted that quantitative findings are the only important products of a particular school and that the theoretical or formal systems developed from the data are of such little importance it is a waste of time to consider them. One practical objection to such statements is that it is inconceivable the Pavlovian school would have invested such a vast amount of time and labor in the specific type of experiment which seems to have held the center of its attention over a period of two decades, except for the underlying theoretical assumptions. The writer can not subscribe to the view that the importance of a project can be estimated by the quantity of data piled up. Because these extensive irradiation investigations were planned upon an invalid assumption, a faulty analogy, their significance is almost wholly immediate and limited. Of greater importance is the fact that the theoretical approach prevented this laboratory with all its magnificent resources from attacking more significant problems.

The limitations of the Pavlovian school are epitomized in its curious, almost obsessive insistence upon a purely physio-

logical point of view to the exclusion of all other considerations. In the writer's opinion, an exclusively physiological system is inadequate to formulate any comprehensive account of behavior in either dogs or men. It is the thesis of this paper that, at the present time, the formulation of behavior must fall back upon a psychobiological point of view. The science of physiology is as yet far too incomplete to permit a comprehensive account of the more complex integrations of man and the higher animals. There is good reason for believing that the narrow physiological viewpoint of the Pavlovian school, through oversimplification, has actually obscured the real complexity in the material with which it has occupied itself and has thereby delayed progress in the understanding of the fundamental features of such phenomena. This does not mean that physiological data are to be ignored. They should be utilized whenever they can offer a legitimate contribution to the understanding of a problem. It should not be overlooked that the Pavlovian methodology has contributed a somewhat novel approach to such problems as sleep, neuroses, and learning. It has provided a superior technique for studying the correlation of structure and function. But it should be recognized that while these contributions provide a point of attack for further research, the collapse of the Pavlovian irradiation hypothesis nullifies the present explanations of these phenomena.

Consider the phenomena of sleep as found in connection with conditioning experiments. A stimulus such as a tone is invariably followed by food. Soon the tone tends to evoke a salivary response even though unaccompanied by food. If, when this reaction is well established, the experimenter continues to give the tone signal but does not give food following the tone, many dogs will go through a period of excitement. They may bark, may show a general increase in restless activity, may strain at the tether. After a period of this behavior, certain dogs show a different pattern of reaction which appears to be initiated by the conditional signal. The dog may rest his head on an adjacent stand, may frequently—perhaps invariably, over certain periods of time—close his

eyes. And, according to Pavlov, although the writer has not had the opportunity of observing this last reaction, the animal may sag in his harness. It is an interesting, and probably a rather significant fact that when a dog is frustrated, *e.g.*, when a piece of food is held just beyond his reach, the periods during which he struggles to obtain the food are followed by quiescent stages in which the animal may lie down and put his head between his paws, may even close his eyes. The closing of the dog's eyes would appear to be a biologically adaptive mechanism in that it temporarily removes an unattainable object and thus conserves his energy until chance would tend to modify the situation so that it might be easier to obtain the food. As legitimate neurophysiological data are discovered they should be utilized in describing such phenomena. Until the time that such facts are uncovered there would seem to be little that can be gained by a mere substitution of terms.

Perhaps the greatest contribution of the Pavlovian school is the impetus it has given to investigations which utilize the conditioning technique. A somewhat more debatable question is whether the theories and applications which have recently been developed from the fundamental Pavlovian concepts represent a permanent contribution, apart from the admirable impetus they have given to research in this field. These theories are based upon such Pavlovian concepts as external inhibition, internal inhibition, disinhibition, inhibition of delay, generalization, differential inhibition, etc. In his extremely valuable review of the phenomena of conditioning, Hull makes the following statement:

> A second hypothesis concerning the relationship of the conditioned reflex to the higher adaptive processes conceives the former as a comparatively simple type of learning which, through its simplicity, enables the basic learning mechanism to be laid bare with relative ease. A brilliant example of such an outcome is seen in the discovery of the complex phenomena of internal inhibition. According to this hypothesis, internal inhibition with its various and complex characteristics should be found present, though hidden as usual, in ordinary habits. . . .
>
> A concrete illustration may serve to make this point clear. In order to test the hypothesis, the writer . . . ran albino rats in a

42-foot straight alley in which the speed of running of each successive 5-foot section could be accurately determined. It was found that if a slight sound was made the animals slowed their pace markedly, which agrees perfectly with the principle of external inhibition (Pavlov . . .). It was also found that when the food reward was taken away these animals ran progressively more slowly, thus manifesting a weakening excitatory tendency, which is exactly what would be expected on the principle of experimental extinction. A period of 24 hours, however, regularly produced an almost complete recovery of speed of running, which is precisely what one would deduce from the principle of spontaneous recovery. Moreover, there was evidence that each destruction of the running-tendency took place more promptly than the preceding one, which again conforms to expectation on the hypothesis that the same principles are operative in ordinary habit structures as in conditioned reactions (6).

What does the term 'experimental extinction' convey, as used in the above example? The rats learned to run to the goal when they were rewarded with food. Being adaptive organisms, once the food was taken away they began to alter the primary habit. Since it takes time to unlearn a well established habit, the process of modification was not complete within one day. Upon being tested the succeeding day their 'spontaneous recovery,' *i.e.*, incomplete alteration of the original habit led to errors in the unlearning process. This type of backsliding is not a new discovery. It is highly questionable whether the terms internal inhibition, experimental extinction, and spontaneous recovery can be employed in such discussions without conveying the physiological connotations intended by their original sponsor.

An example of the lengths to which this type of analogizing can go is indicated by Lepley's recent formulation of a mechanism to account for the curve of forgetting and related phenomena of learning (8)—a formulation, incidentally, which has received a rather surprising endorsement. It is based upon the assumption that the remote excitatory tendencies first noted by Ebbinghaus can be identified with the trace conditioned reflexes as studied in the Pavlovian laboratory. It utilizes the concepts of internal inhibition and spontaneous

recovery as explanatory principles. Its author appears, however, to have confused conditioning which is established to a span of time, with phenomena which are primarily based upon sequence—assuming that rote learning of nonsense syllables has a significant relation to trace conditioning in dogs. To prove that the analogy is at all justified it would be necessary to demonstrate that the tendency of a stimulus near the beginning of a series to evoke a verbal reaction belonging at a later point in the series is essentially a matter of time interval, *e.g.*, rate of recitation at which the original series was practiced. Moreover, the phenomena of response to a specific passage of time is at present so poorly understood that it would not add much in the way of explanation to assume that the two phenomena are similar.

The keystone of Lepley's theory is that remote inhibitory tendencies weaken more rapidly than remote excitatory tendencies. In the reference to Pavlov cited by Lepley (11, pp. 58–59), there is the following statement: "Left to themselves extinguished conditioned reflexes spontaneously recover their full strength after a longer or shorter interval of time, but this of course does not apply to conditioned reflexes which are only just in process of formation. Such reflexes, being weak and irregular, may require for their recovery after extinction a fresh reinforcement by the underlying unconditioned reflex. However, all those conditioned reflexes which have been fully established invariably and spontaneously return sooner or later to their full strength." It is evident from this abbreviated quotation that the general statement "inhibitory tendencies are much less stable than are excitatory tendencies," requires qualification. The fact that a well established conditional response is not broken up by a brief period of unlearning in no way justifies Lepley's deduction. To give the statement validity it would be necessary to determine experimentally whether excitatory tendencies weaken more slowly than inhibitory tendencies *to which they have been equated.*

The writer has had extensive experience with delayed conditional salivary secretions, which Lepley asserts are fundamentally the same as trace responses. Such reactions

may be retained for months, and are probably to be understood from the psychobiological standpoint as adaptive reactions. That is to say, the dog has learned the length of time which routinely separates stimulus and food. He avoids grossly premature anticipatory reactions, may not salivate until just a moment before the food arrives. This phenomenon tends to develop with dogs that have been trained over a long period of time according to a consistent routine. It may, however, develop very early in certain cases so that such dogs may be classified, erroneously, as slow learners because their delayed conditional secretion is masked by the unconditional salivation. The writer has never been able to 'disinhibit' this particular 'inhibition of delay' if indeed there exists such a neural state in these cases.

The writer would object to the implicit assumption that what is interpreted as 'inhibition of delay' in the case of the dog can be taken as a starting point for a law involving a postulated remote inhibitory tendency in rote learning. It should first be demonstrated that there are active inhibitory tendencies in rote learning. It must then be shown that the remote excitatory tendencies when, and if released from their respective influences actually conflict with the immediate excitatory tendencies so as to result in mutual destruction. Finally, these events must be shown to bear the proper time relationships to the curve of forgetting which they are supposed to explain.

From the writer's reference to the difficulty of modifying a well established delayed conditional salivation it should not be assumed that other delayed responses are equally stable. In the case of the conditional electrodermal response, Switzer (13) has presented data which he accounts for on the principles of Pavlovian disinhibition. The writer does not question the findings of this particular experiment. It might be asked, however, whether the Pavlovian formulation actually contributes anything to the understanding of the data or the problem, or whether it is merely a transposition of the old association formulae into new terms.

This discussion might be rounded out with a final consideration of the implications of the exclusively physiological viewpoint espoused by the Pavlovian school. Denny-Brown (**3**) has pointed out the inherent weakness in identifying the conditional response with the reflex upon which it is established. Somewhat earlier, Guthrie (**5**), discussing this problem from the psychobiological standpoint, stated: "This does not at all mean that learning is described in terms of conditioned reflexes. This phrase assumes a fixed unit of behavior which is organized in stereotyped form. It tends to obscure the fact that the behavior of an intelligent organism at any instant is a resultant of the total stimulus situation including internal stimuli. Reflexes and responses are never twice alike, because the total stimulus situation is never repeated. We may, at the outset, distinguish a theory of learning in terms of conditioning from a theory of learning in terms of conditioned reflexes. We have made no assumptions concerning the elementary acts which comprise behavior." Upon the basis of six years of daily experience with the traditional salivary conditioning technique the writer has been led to feel that this point can not be emphasized too strongly. There is, commonly, a latent period of several seconds separating the conditional signal and the initial displacement of saliva in the salivary duct. Many dogs exhibit a number of very significant reactions during this span of time suggesting that the expression 'simple conditioning,' in many cases is entirely inappropriate.

The primary assumption of the consistent objectivist is that the sample of activity which he records is, at the least, a statistically reliable index of the process under investigation. But there is increasing evidence that the correlation between overt behavior and implicit pattern is at best imperfect. In the writer's opinion, the implicit aspect would appear to be the more fundamental in all instances (**10**)—including those cases where its nature can not be ascertained. This statement is intended as a frank admission that in comparative psychology and in human behavior as well, the experimentalist at times may be blocked in studying the most significant

aspect of a particular process. To affirm that the overt reactions are merely indicators of a relatively inaccurate sort, tends to prevent the extreme in oversimplification. The difficulties which, three decades ago, gave rise to the movement to end all subjectivism remain to be faced. It is questionable whether anything is now to be gained by ignoring the difficulties, perhaps indeterminateness, of certain vital problems of behavior. It would seem, rather, that a clear recognition of the limitations of present techniques will ultimately result in more lasting progress.

BIBLIOGRAPHY

1. Bass, M. J. and Hull, C. L., The irradiation of a tactile conditioned reflex in man, *J. Comp. Psychol.*, 1934, **17**, 47–65.
2. Boring, E. G., A history of experimental psychology, New York: The Century Company, 1929, pp. 619–620.
3. Denny-Brown, D., Theoretical deductions from the physiology of the cerebral cortex, *J. Neur. & Psychopath.*, 1932, **13**, 52–67.
4. Gundlach, R. H. and Herrington, G. B., The problem of relative and absolute transfer of discrimination, *J. Comp. Psychol.*, 1933, **16**, 199–206.
5. Guthrie, E. R., Conditioning as a principle of learning, Psychol. Rev., 1930, **37**, 412–428.
6. Hull, C. L., Learning: II. The factor of the conditioned reflex, In 'A Handbook of General Experimental Psychology' (edited by C. Murchison), Worcester, Mass.: Clark University Press, 1934, pp. 382–455.
7. ——, The conflicting psychologies of learning—a way out, Psychol. Rev., 1935, **42**, 491–516.
8. Lepley, W. M., A theory of serial learning and forgetting based upon conditioned reflex principles, Psychol. Rev., 1932, **39**, 279–288.
9. Loucks, R. B., An appraisal of Pavlov's systematization of behavior from the experimental standpoint, *J. Comp. Psychol.*, 1933, **15**, 1–45.
10. ——, Experimental delimitation of neural structures essential for learning, *J. Psychol.*, 1936, **1**, 5–44.
11. Pavlov, I. P., Conditioned reflexes, London: Oxford University Press, Impression of 1928, pp. xv + 430.
12. ——, Lectures on conditioned reflexes, New York: International Publishers, 1928, pp. 414.
13. Switzer, St. C. A., Disinhibition of the conditioned galvanic skin response, *J. Gen. Psychol.*, 1933, **9**, 77–100.

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THE RELATIONSHIP OF RETROACTIVE INHIBITION, RETROGRADE AMNESIA AND THE LOSS OF RECENT MEMORY

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One of the steps by which science proceeds is to subsume two or more topics under one explanatory principle so that what were apparently unrelated phenomena appear as different manifestations of the same more basic mechanism. This paper is an attempt to relate in this manner: 1. retroactive inhibition, 2. retrograde amnesia, and 3. the comparative inefficiency of recent as compared with remote memory in certain psychoses, for instance in psychosis with cerebral arteriosclerosis or in Korsakow's psychosis.

It is immediately evident that the types of activity mentioned are similar in that they all lie in the field of memory, they are all concerned with losses of memory, in each case the memory material lost is that which was registered during the temporal period just preceding the event which caused the loss. The most noticeable difference between the three would seem to lie in the fact that the periods of time covered by the losses vary from a few minutes to a period of years.

In the case of retroactive inhibition the underlying neural change which accompanies the change in behavior must be the ordinary type of change which accompanies all psychological events. In the cases of retrograde amnesia and the loss of recent as compared with remote memory there may be some question as to whether the accompanying change in the nervous system is this same 'psychological' type of change or whether it is the grosser change involving destruction of the nervous tissue which is found after certain diseases, blows on the head, embolisms, etc. Let us refer to the type of change in the neural tissues which accompanies psychological events as the 'nondiscernible' change and to the change which is a

destruction of the nervous tissue as the 'discernible' change (*e.g.*, discernible at autopsy).

In retrograde amnesia the cause of the amnesia would seem to be associated with some trauma. The trauma is often 'physical' but may be 'psychic.'[1] Bleuler and Brill (1, p. 101) say, "Amnesias are also not uncommon after purely affective explosions in psychopathic or in really insane patients." Since, however, every physical trauma may be considered to be accompanied by a psychic trauma, we may say that every case of retrograde amnesia has an element of psychic trauma in its origin. The change in the organism with psychic trauma would seem to be the nondiscernible change mentioned above.

The loss of memory for recent as compared with remote events might involve either the discernible or the nondiscernible type of change. It is true that both microscopic and macroscopic changes have been noted at autopsy of the brains of senile dements, and that at least some psychiatrists and neurologists refer the memory losses to these discernible changes. Rosanoff (13, p. 39), for instance, says, "The disappearance, under the influence of some pathological cause, of impressions previously acquired, constitutes what we have termed amnesia of conservation. This destructive, and consequently incurable, form of amnesia is the principal factor of certain types of dementia, . . ." We understand from the use of the terms 'pathological,' 'destructive,' and 'incurable' that the change referred to is the type we have called the discernible change. But on the other hand it is also probable that not all cases of loss of recent memory show an organic destruction of brain tissue discernible at autopsy, and it is further true that this loss of memory occurs in conjunction with a psychosis which involves other behavioral changes in addition to the memory loss. These facts suggest that the loss of recent memory may occur in conjunction with the 'psychological' type of change, or nondiscernible change.

[1] I do not like the apparent dichotomy between 'physical' and 'psychic' suggested here but use the terms because they are the terms in which such matter is usually discussed, *e.g.*, see Warren (16) under 'Trauma,' and Dorland (5).

To recapitulate, then, the retroactive inhibition involves the nondiscernible change, retrograde amnesia involves the nondiscernible and perhaps the discernible change, and the loss of recent as compared with remote memories presumably involves the nondiscernible change. The nondiscernible change is the type common to all three of the phenomena and if our thesis, that these phenomena are manifestations of the same underlying mechanism, is correct, then the nondiscernible change is the type which characterizes the underlying mechanism.

Now let us examine the phenomena with respect to the traditional division of an act of memory into registration, retention and recall. Hull (**8**) has found a deficiency in registration in certain psychoses. The study did not include those psychoses in which the loss of recent as compared with remote memory occurs, but other writers have said that there is a deficiency in registration in these latter psychoses (*e.g.*, Bleuler and Brill (1, p. 98)). However, this defect in recent memory involves in many cases the loss of material which was once well known, which indicates that it was once registered, whether with difficulty or with ease. Since this is so and since there is usually no deficiency in the registration in the cases of retroactive inhibition and retrograde amnesia, we conelude that none of the three phenomena can be explained as being due to a deficiency in registration.

With regard to retention and recall it is impossible to distinguish between the relative weights of the two in cases where material can not be remembered. If the material is not remembered we can not say whether the fault is due to the patient's failure to retain or failure to recall. Where the material requested is produced by the patient, we can not say how much of the reproduction is due to the strength of the retained trace and how much to the height of the threshold for recall. It seems conceivable that some conditions might lower the threshold of recall so as to compensate for a lack of strength in the retention.

In spite of the fact that we can not in general distinguish between the retention and the recall segments of an act, we

can sometimes decide that material is retained but not recalled. When a person is on one day unable to produce certain material but is able at a later time and with no intervening relearning to reproduce it correctly, we can not but conclude that on the first occasion the material was retained but could not be recalled. Let us see if we can differentiate between retention and recall in the case of the three phenomena under discussion.

Studies in retroactive inhibition (*e.g.*, McGeoch, **12**, and Britt, **2**, especially p. 423) demonstrate that the learned material which is 'inhibited' is relearned at a saving. This indicates that although the material can not be recalled it is to some extent retained. There is, then, some loss in the ability to recall. That the inability may lie at the point of recall has already been suggested by Skaggs and by McGeoch. Skaggs (**15**, p. 413) suggests "the establishment of wrong associative tendencies which operate at the time of recall." McGeoch (**12**, p. 41) says, "According to the third possibility, the temporal locus of negative transfer is during the measurement of retention . . . the third possibility is the most acceptable." 'During the measurement of retention' is, of course, the time of the recall.

In the case of retrograde amnesia there is evidence that in at least some cases the difficulty is in the field of recall rather than in that of retention. Hull (**9**) cites a case of retrograde amnesia in which the memories of the events within the period covered by the amnesia were recalled under hypnosis. He suggests, however, that since there was no check on the accuracy of the material recalled in the trance this case must be treated carefully. Schilder (**14**) cites thirty-five cases of which two showed retrograde amnesias with a subsequent recovery of the lost memories. Both of these (Albert G., pp. 161–2; and J. T., pp. 162–3) are like Hull's case in that there was no reliable check on the accuracy of the recovered memories.

In the matter of recent and remote memories we find that sometimes a patient will be unable to remember a certain fact today but will be able to remember it correctly tomorrow.

We quote from Bleuler and Brill (1, p. 280). In discussing 'Arteriosclerotic Insanity' the authors say, "Gradually disturbances of memory, especially for recent experiences, become more pronounced, at first only on certain occasions, as on seeking a name, etc.; later it is more generally noticeable, but always oscillating up and down." Henderson and Gillespie (7, p. 338), in discussing psychosis in cerebral arteriosclerosis, say, "A failure of memory commences, first of all especially for names. The failure is a capricious one, the same name being recalled at one time, and forgotten at another. The memory defect becomes more general, and gradually extends from recent to remote events."

These statements suggest that in at least some cases in which there is a loss of recent as contrasted with remote memory the fault is to be found in the recall rather than in the retention of the material. This is the view advanced by Bleuler and Brill (1, p. 99). In discussing recent and remote memory these authors say, ". . . hence in organic hypomnesia one deals with a disturbance of memory that involves the more recent experiences disproportionately stronger than those lying further back, even though what is experienced during the disease is for various reasons often particularly poorly remembered . . . the main disturbance is probably in the ability to recall . . . disturbances of the retention of the engrams are not significant in psychiatry."

We may conclude, then, that the three phenomena which we are discussing are similar to one another in that they may be due to inability to recall rather than to inability to retain.

Now it seems that perhaps this inability to recall may be associated with some emotional or affective disturbances. In psychoanalysis we find a whole system of therapy devoted to the release of emotional tensions which prevent recall.

In retrograde amnesia there is a psychic trauma which involves a strong unpleasant emotional tone. In the psychoses in which there is an impairment of recent memory there is undoubtedly a strong emotional element. Bleuler and Brill (1, p. 287) say in discussing senile dementia, "Memory pictures of a pleasant character can be more easily revived

than the unpleasant." The general characteristic emotional states in these psychoses are of an unpleasant type. Church and Peterson (4, p. 820) under the heading 'Senile Dementia' say, "The underlying mood is often melancholic; an exalted mood is extremely rare. Changeability with irritability is perhaps the most usual affective condition." Jelliffe and White (10, p. 737) in listing the chief symptoms of cerebral arteriosclerosis mention 'increased emotionalism and irritability.'

In the case of retroactive inhibition there is evidence to show (6) that unpleasant interpolated material has an inhibitory effect and pleasant interpolated material a facilitative effect on the material of the original learning. Also (3) pleasant material shows a smaller amount of retroactive inhibition by indifferent material than does unpleasant material. These same authors and others (11) showed that pleasant material is learned and retained more easily than unpleasant. We find, then, that there is an emotional element suggested in the failure to recall in each of our three phenomena and that this emotional or affective element is usually a negative or unpleasant one.

The next question concerns the time in the memory process at which the emotional element enters. Studies of memory, learning, etc., give some indication that unpleasant material is less easily learned and retained than is pleasant material, and indifferent material is more subject to retroactive inhibition by unpleasant material than by pleasant. That is to say, the unpleasant affective tone may be present at the time of registration or may come in after the registration and before the recall, *i.e.*, during the period of retention. In retrograde amnesia the unpleasant affective tone certainly comes in between the time of the registration and the time of the recall—during the retention period. In the case of the psychoses the unpleasant tone comes in during the retention period and is also present at the time of the recall. In each of our three phenomena, then, the unpleasant tone may be acquired during the time of the retention.

We can now expand the statement on page 339 as follows: The three phenomena under discussion are similar in that (1) they all lie in the field of memory; (2) they all involve losses of memory; (3) in each case the memory material lost is that which was learned during the temporal period immediately preceding the event which caused the loss; (4) each phenomenon would seem to be accompanied by a 'non-discernible' change in the nervous system of the type which accompanies such psychological processes as learning; (5) each may be an inability to recall rather than an inability to retain; (6) each may involve an emotional tone; (7) this emotional tone may be unpleasant; and (8) this tone may be acquired during the period of the retention in each case.

BIBLIOGRAPHY

1. Bleuler, E. and Brill, A. A., Textbook of psychiatry, New York: Macmillan, 1924.
2. Britt, S. H., Retroactive inhibition: a review of the literature, *Psychol. Bull.*, 1935, **32**, 381–440.
3. Bunch, M. E. and Wientge, K., The relative susceptibility of pleasant, unpleasant, and indifferent material to retroactive inhibition, *J. Gen. Psychol.*, 1933, **9**, 157–178.
4. Church, A. and Peterson, F., Nervous and mental diseases, Philadelphia: W. B. Saunders Co., 1921.
5. Dorland, W. A. N., American illustrated medical dictionary, Philadelphia: W. B. Saunders Co., 1935.
6. Frank, J. D. and Ludvigh, E. J., The retroactive effect of pleasant and unpleasant odors on learning, *Amer. J. Psychol.*, 1931, **43**, 102–108.
7. Henderson, D. K. and Gillespie, R. D., Textbook of psychiatry, London: Oxford University Press, 1930.
8. Hull, C. L., Formation and retention of associations among the insane, *Amer. J. Psychol.*, 1917, **28**, 419–435.
9. ——, Hypnosis and suggestibility, New York: D. Appleton-Century Co., 1933.
10. Jelliffe, S. E. and White, W. A., Diseases of the nervous system, Philadelphia: Lea and Febiger, 1935.
11. Jones, H. E., Emotional factors in learning, *J. Gen. Psychol.*, 1929, **2**, 263–272.
12. McGeoch, J. A., Studies in retroactive inhibition: I. The temporal course of the inhibitory effects of interpolated learning, *J. Gen. Psychol.*, 1933, **9**, 24–43.
13. Rosanoff, A. J. (Ed.), Manual of psychiatry, New York: John Wiley & Sons, 1927.
14. Schilder, P., Psychic disturbances after head injuries, *Amer. J. Psychiat.*, 1934, **91**, 155–187.
15. Skaggs, E. B., A discussion on the temporal point of interpolation and degree of retroactive inhibition, *J. Comp. Psychol.*, 1933, **16**, 411–414.
16. Warren, H. C. (Ed.), Dictionary of psychology, New York: Houghton Mifflin Co., 1934.

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A NOTE ON SYSTEM-THEORY

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Mr. Hilgard's interesting and acute article[1] contains a footnote which calls for a certain mild expostulation. While praising, very flatteringly, my account of the process of conditioning,[2] he adds a reference to my "excursions into system-theory," which, he says, do not detract from the merits of what he is good enough to praise. I feel impelled to submit the following considerations.

(1) As psychologists, we have to come to terms with the notion of system. Whether we wish it or not, both biologists and psychologists of the highest rank are actually using the term in their description of living creatures. Instances are (*a*) Bayliss and Pavlov, (*b*) the Gestalt group and the author of the article immediately following Mr. Hilgard's in this Review.[3] If psychologists are to use the term at all, we must examine its implications, both from the general point of view and, specifically, for such special problems as that of the conditioned reflex. This I attempted to do in my "excursions into system-theory."

(2) "To say that organisms go about achieving four-dimensional integrations is to say only that organisms live in space and time. This is irrefutable, but it leaves the detailed problems . . . unclarified" (Hilgard). Here I must differ. Newton might have said that organisms live in space and time. The implications are that a living creature is (1) a three dimensional static *thing*, which (2) does something in an independent time dimension. ("We have been in the habit

[1] E. R. Hilgard, The nature of the conditioned response, II, PSYCHOL. REV., 1936, 43, 547–564.

[2] G. Humphrey, The nature of learning, New York: Harcourt Brace; London: Kegan Paul, 1933.

[3] E. G. Boring, Psychophysiological systems and isomorphic relations, PSYCHOL. REV., 1936, 43, 565–587.

of treating time as an independent continuum." [4]) The living creature is essentially an event, a *process*, and cannot be treated in the Newtonian manner without first assuming it is dead. Thus: "Murder is a prerequisite for the absorption of biology into physics as expressed in the traditional concepts" (Whitehead). Organisms are perpetually integrating, four-dimensional systems, living in a (four-dimensional) space-time continuum. The problem of learning is essentially bound up with our treatment of time. As long as we "treat time as an independent continuum," what we call learning will appear as a special problem. It is not a special problem, but is a special name for the integration of similar situation-elements, necessitated by treating the time dimension as independent. Thus the concept of integration in a four-dimensional system does effect an important clarification of the general problem of learning, including, of course, the special problems of the conditioned reflex, maze running, negative adaptation, and so on. The detailed problems all become special cases of the one more inclusive principle.[5] What is ordinarily called integration is a limiting case where the time-factor is arbitrarily neglected, all integration being of course strictly four-dimensional.

Thus the conception does tie-in with a multitude of experimental data, but not quantitatively, because, as Hilgard points out, we have not yet the "interchangeable constants necessary for generalized formulas" (p. 554).[6]

(3) I find myself, then, very much in agreement with Hilgard's own conclusion. "By taking as a point of departure the relationships discovered in experiments . . . it is possible to construct a series of statements regarding events in conditioning experiments. . . . These statements . . . *are at present the only possible laws with a solid empirical basis*" [7] (p. 561). Only I feel I must add: "such laws will concern the behaviour

[4] A. Einstein, Relativity, London: Methuen, 1924, p. 56.

[5] G. Humphrey, The nature of learning, New York: Harcourt Brace; London: Kegan Paul, 1933, Chap. V.

[6] In the same way the concept has heuristic value of a non-quantitative kind.

[7] E. R. Hilgard, The nature of the conditioned response, II, PSYCHOL. REV., 1936, 43, 547–564.

of an organism, that is to say, of a living system which constantly perpetuates itself four-dimensionally in a four-dimensional environment; they must ultimately be formulated in such a way as not to assume postulates which physical science has shown to be untenable." The conception of the four-dimensional living system provides a framework in which such empirically determined laws will fit, not a generality from which they can be deduced.

When my general argument was not made clear to so accomplished an experimenter as Mr. Hilgard, I do sincerely feel that my exposition was somewhere badly at fault.

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INTERNAL AND EXTERNAL DETERMINANTS OF DRIVES

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I. Introduction

In Bernard's (2, p. 152) careful evaluation of the issues involved in the instinct controversy he proposed a pruning down of instincts to their inner structure. On this basis he suggested three groups whose organic connections were, respectively: internal, external, and mixed. He pointed out that the tracing of the neural pathways had to await the results of further investigation. In the intervening years much progress has been made in this direction and the problems for future research are more clearly indicated. In the meantime the present-day classification of drives seems to have received its chief impetus not so much from Bernard as from Craig (9). This investigator contrasted 'appetites' and 'aversions.' Appetites were attributed to the *absence* of an 'appeted' stimulus, while aversions resulted from the *presence* of a disturbing stimulus. Although he regarded appetites as dependent upon internal factors, he did not assert a dependence of aversions upon external stimulation. According to earlier usage, the source of the stimulus served to distinguish appetites from instincts. Craig, however, used 'instinct' as a generic category. Instincts as a group he regarded as cyclic, including appetites and aversions as phases. This attempt to include both processes under the same heading led to far-fetched interpretations of such drives as that of escape, which can hardly be said to have an appetitive phase as an essential component.

Recent writers have modified and extended Craig's formulation. Holt (13, Chap. XIV), after tracing the historical development of the dichotomy, offers his own classification, based exclusively on source of stimulation. He

points out that there is not sufficient current recognition of this fundamental division. The distinction is borne out in the manifestations of the drives in the two categories. Reactions to appetitive stimuli are necessarily touched off spontaneously and at more or less regular physiological periods, whereas responses released by external stimuli often go for years without activation. He makes the further point, previously advanced by Jennings (**15**, p. 309ff.), that all driven behavior is in its primary mechanism *avoidance* even though appetitive conduct has the appearance of *seeking* an end.

One of the most completely integrated systems of motivation is that of Tolman (**26**). Like Holt, he recognizes a basic distinction in terms of source of stimulation, but makes the point that even the *external* stimulus, 'consummatory or disturbing,' should be regarded as merely the precipitating cause of the drive, operating against a physiological background which constitutes the predisposing cause (**26**, p. 273). While supporting Craig's assumption concerning the importance of physiological state in both classes of function, Tolman distinguishes appetites from aversions by emphasizing their cyclic nature. In the most recent formulation of his viewpoint (**27**) he describes appetites as demands for positive goal-objects (p. 367) and aversions as demands against negative goal-objects (p. 368), but fully recognizes the more basic demand underlying both categories; namely, the demand to restore the 'quiescence' which has been temporarily disturbed.

A survey of the development of contemporary viewpoints reveals that definite progress has been made by slight accretions to the main current of thinking, rather than by attempts to revolutionize the field. In line with this trend the writers wish to elaborate the present concepts in two directions: first, a clarification of the part played by internal and external stimuli in drives; secondly, a stabilization of the classification presented by relating it more closely to underlying physiological mechanisms.

II. Source of Stimulation as a Criterion

Definition of drives.—The terms 'drive' and 'drives,' as currently employed, have been clearly analyzed by Young (**31**, p. 70ff.). Following Woodworth (**29**), he defines 'drive' as "the physical energy which makes the machine go." By 'drives' he refers either to certain kinds of behavior or to the physiological conditions activating them. A generic definition of drives cutting across both of these meanings might be stated as follows: *A drive is an activity of the total organism resulting from a persistent disequilibrium.*

As a formal definition this is admittedly inadequate. Taken broadly it is coextensive with life itself. In a narrower sense 'activity of the total organism' excludes certain locally regulated physiological processes, such as normal digestion, respiration, and temperature control. Combined with 'persistent disequilibrium' it excludes a number of reflexes which are readily performed, such as blinking, coughing, and withdrawing from easily avoided cutaneous stimuli. Finally it excludes such relatively passive processes as mere perception and 'free' association. But the exclusion is by no means hard and fast. Under special conditions respiration and heat regulation may become drives. The varying duration of a sneeze, as Woodworth (**30**, p. 243) has pointed out, makes its classification difficult, as does the varying duration of cutaneous pain. Micturition and defecation are reflexes for the infant, drives for the older child. Moreover, when we apply our definition to integrative processes of the cerebral cortex, exclusion becomes decidedly arbitrary.

The term 'disequilibrium' also suffers from lack of precision. In a broad sense it covers whatever physical conditions give rise to activity. In some cases, as in nutritive functions, the imbalance may be found in an excess or deficiency in the chemical composition of the blood; in others, as in extreme temperature changes, it may be said to exist between external and internal conditions. In such cases the nature of the disturbance is more or less verifiable. It may, however, be set up in the central adjustors by associative processes, as in much of the purposive behavior characteristic

of higher forms. Here for the present the state of disequilibrium must be assumed rather than described.

The enumeration of drives.—Instead of attempting to refine our tentative definition it seems more fruitful to enumerate the specific drives with which the present paper is concerned. To begin with, we shall not attempt to cover the particular directions of activity which are laid down in each individual's habit-system. In the course of experience the factors underlying one's goals and disturbances become exceedingly obscure. Certain dispositions, however, appear almost universally among a wide variety of mammals regardless of differences in previous experience. Of these the following are representative and will be here considered. They are designated by the objects or activities biologically adequate to restore equilibrium: Water, Food, Rest, Activity, Exploration, Mate, Progeny (suckling), Attack and Ascendance, Escape and Submission.

The list does not pretend to be exhaustive. Otherwise it would include drives to air and to moderate temperature under abnormal conditions and to urination and defecation at a certain stage of development. The reader may miss other familiar categories. Tolman (**26**, p. 290) makes a distinction between 'first-order' and 'second-order' drives, designated by Young (**31**, p. 255), 'primary' and 'secondary,' hinging on whether or not a differential physiological mechanism is known. So little is known of the unlearned origin of such secondary drives as 'gregariousness' and 'imitativeness' that present discussion would be futile. 'Exploration,' which may be considered a forerunner of 'curiosity,' seems closely linked with a general drive to activity, at least in its earliest expression, but is tentatively given separate status. 'Ascendance' (self-assertion) and 'submission' (self-abasement) are here treated as milder expressions of attack and escape, respectively. In extreme cases they may become chronic ways of reacting to social stimuli. In spite of such controversial issues an analysis of these basic drives will indicate something of the matrix out of which more complex needs are specialized.

Classification of drives.—Our next task is to organize the drives according to their fundamental similarities and differences. We seek a criterion which will serve two functions: first, to bring into relief underlying physiological mechanisms; secondly, to point the way to further research. These considerations suggest, as the most useful criterion, *the relative importance of internal and external stimuli.* The classifications already proposed by Craig, Holt, and Tolman for the primary drives fit readily into this framework, the 'appetites' depending primarily on internal excitation, the 'aversions' on external. Since the former group are initiated by cyclically recurrent, biological needs, the writers will refer to them as 'vegetative,' reserving the older term 'appetite' for the conditioned arousal of a vegetative drive in the absence of tissue need. For the other group of drives, in which the most conspicuous organic changes *follow* external stimulation, we shall use the term 'emergency.' This does not mean that *all* internally aroused activity is vegetative. Certain pathological conditions, such as intestinal pain, fever, or hypoglycemia, evoke emergency reactions. On the other hand, all external stimuli to activity are not sufficiently intense to arouse emergency behavior. This is true of the exploratory and certain other secondary drives. But for the primary drives of the healthy organism in the environment to which it is biologically adapted the proposed grouping is on the whole valid.

III. Physiological Correlates

The correlation between organic disequilibrium and driven behavior may be found throughout the course of evolution. Claude Bernard long ago pointed out the necessity of maintaining constancy of the internal *milieu* within very narrow limits, a principle recently elaborated in Cannon's concept of 'homeostasis'(5). When it is disturbed, the organism is driven until the balance has again been restored. The mechanism for maintaining or regaining such balance is deeply embedded in the phylogenetic history of the race. The *internal* environment of higher forms has its prototype

in the *external* watery medium surrounding the earliest forms. Before the evolution of nervous tissue there was a physico-chemical regulation of the balance among antagonistic groups of electrolytes within the individual cells. More centralized regulation of such basically antagonistic processes is found at later stages in endocrine and nervous controls. The 'autonomic' nervous system, which innervates the relatively autonomous viscera, consists of two divisions, the thoracico-lumbar or sympathetic, and the cranio-sacral, whose functions are to a large extent antagonistic, paralleling the antagonism between groups of electrolytes. Certain endocrines exert chemical influences which correspond to and supplement the nervous influences. For example, adrenin and thyroxin are synergic with the sympathetic, while the hormones of the parathyroid and thymus glands have opposing effects. The maintenance of health implies a balance between the two branches, while prolonged disturbance in either direction is associated with a pathological condition. Ludlum and McDonald (**20**) have elaborated this viewpoint and have even gone so far as to list sympathicotonic and vagotonic diseases.

As applied to the basic drives, the concept of autonomic balance permits a similar grouping. When tissue needs occur under normal conditions they apparently set up temporary disequilibria in favor of the cranio-sacral division. In so far as they involve only a single organ system they occur against an autonomic background which remains, on the whole, balanced. Examples are the marked activity of the vagus in hunger and of the sacral paths in sexual excitement. Correlated with these objective manifestations are the strongly affective specific cravings. When, on the other hand, external crises arise they require the resources of the total organism. By the breakdown of the autonomic balance in the direction of the widely distributed 'sympathetic,' these resources are mobilized and the organism as a whole is prepared for violent reaction either of escape or attack. This stirred-up objective condition in the viscera is accompanied by the stirred-up subjective condition characteristic of emotion. Whether the specificity of the emotional experience

is dependent upon the arousal of the peripheral changes or whether it is primarily due to stimulation of the appropriate hypothalamic centers is still one of the unsettled controversies in psychology. At the present time this much can be provisionally stated: the external stimulus arouses a center, probably in the hypothalamus, either reflexly or after cortical irradiation. This stimulation results in the arousal of smooth muscle changes via the sympathetic which may or may not be specific, and also in striped muscle changes which are more probably specific. The emergency drives are thus distinguished from the vegetative by sympathetic as contrasted with cranio-sacral predominance.

It should also be pointed out that the two main classes of drives may be differentiated by the temporal sequence of events from the initiating stimulus to the ensuing activity. In the emergency drives the first step is the external stimulation. Then follows the excitation of the sympathetic nerves and synergic endocrines, which eventuates in a disturbed blood chemistry. In those vegetative drives, on the contrary, whose function is the preservation of the individual, the disturbance in the blood condition is primary and is secondarily communicated to the nervous mechanisms involved. Those vegetative drives whose function is the preservation of the species afford an interesting transition. Here the origin of the disturbance is to be found in the chain of endocrine sequences associated with the mating and suckling drives. These changes are reflected in the blood stream, through which they ultimately affect the appropriate autonomic mechanisms. Since in the mammalian male there is no cyclic sequence of endocrine events, the blood stream condition is more constant than in the female, and hence external stimulation is of greater relative importance in initiating the drive. This was borne out by Warner's (**28**) study of sex behavior in the white rat. Thus through the reproductive drives we have partially bridged the gap between the categories of internal and external arousal.

It should be noted that this physiological distinction between vegetative and emergency drives is dependent on the

degree of disequilibrium. In all drives disturbance in the internal environment, within narrow limits, initiates or reinforces behavior directed toward the removal of the irritating source and the restoration of equilibrium. We have seen that in the vegetative drives under normal conditions this is accomplished by local mechanisms, whereas in the emergency drives the resources of the total organism are mobilized to meet the more extreme crisis. It should be pointed out that under conditions of abnormal prolongation certain vegetative drives can no longer be handled locally but require calling out the organism's reserves. This entails an overflow into the sympathetic, such as occurs, for example, in asphyxia. Such sympathetic involvement in frustrated vegetative drives has been recently recognized by Elliott and Bousfield (10) in a paper suggesting proprioceptive and sympathicoadrenal mechanisms as basic in motivation. With reference to the hunger drive they state: "When the restlessness and activity resulting from the operation of the proprioceptive mechanism does not bring the organism into contact with food, the second mechanism enters the picture and serves the end of putting the organism into an emergency condition." If conditions eliciting certain drives become extreme, so that the margin of safety is transgressed, attempts at adjustment may cease and collapse ensue. A striking example is the reaction to cold reported by Barcroft (1, p. 81), in which, after prolonged exposure, he noted a sudden shift from shivering to a 'beautiful feeling of warmth' accompanied by a change in the whole mental outlook. This was interpreted as a collapse reaction which would have led to ultimate death had not the experiment been ended in time.

The psychological significance of the autonomic mechanisms is profound. In all cases where the internal *milieu* has been upset the ultimate strain is on the subcortical centers engaged in regulating the driven behavior which eventually restores equilibrium. The more recently evolved forebrain centers provide a mechanism for the modification of the primary drives and for their integration with the other functions of the organism. It is these integrating centers which

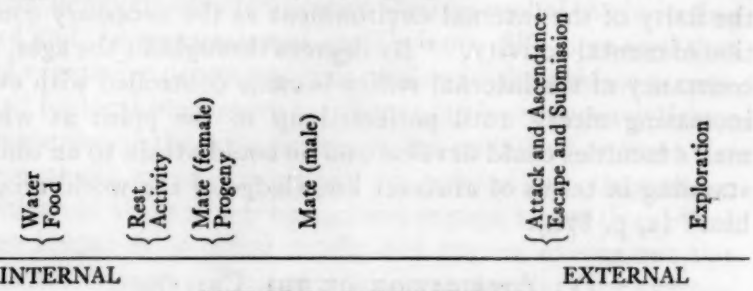
are most quickly affected if the environment alters beyond physiological limits. This observation led Barcroft to regard the fixity of the internal environment as the necessary condition of mental activity. "By degrees throughout the ages, the constancy of the internal *milieu* became controlled with ever-increasing nicety until perfected up to the point at which man's faculties could develop and he could attain to an understanding in terms of abstract knowledge of the world around him" (1, p. 87f.).

IV. Application of the Criterion

It has been shown that the two-fold classification of drives is reflected, in the main, in their physiological mechanisms. Growing out of their relative dependence on internal and external stimulation, vegetative and emergency drives differ in the following ways: (1) the vegetative drives are on the whole cyclic, the emergency non-cyclic; (2) the vegetative drives involve a disturbance of the normal autonomic balance in favor of the cranio-sacral division, the emergency in favor of the sympathetic; (3) the order of initial processes is from chemical to neural in the vegetative drives, from neural to chemical in the emergency.

However, there is reason to believe that a simple dichotomy is unsound. In the first place, Tolman (26) has already emphasized the rôle of physiological predisposition in the arousal of emergency reactions. As to the vegetative drives, comparatively little is known of the potency of specific goal-objects prior to learning. In the second place, it was pointed out that the male sex drive seems to occupy a special position. A more valid method is to construct a hypothetical scale running from purely internal determination at one end to purely external at the other, with varying proportions between. We may then attempt to locate the drives according to the relative effectiveness of the two kinds of stimuli. This, of course, is a project for future consummation. Many experiments must be performed before the criterion here proposed can be applied in anything like a quantitative

manner. In the meantime a tentative arrangement of drives is suggested in the accompanying figure.



Those drives subserving imperative tissue needs, as for water and food, belong close to the internal end of the scale. The mechanisms of hunger and thirst have been elsewhere described, notably by Cannon (**4**). Although the chemical details are still obscure, the dependence on tissue depletion is unquestioned. The needs for rest and for activity bear a reciprocal relation to each other but should be considered separately, since each is a positive drive, not merely a negation of the other. Prolonged activity leads to relaxation, often accompanied by sleep, or, in more restricted processes, to a change of occupation. Johnson, Swan, and Weigand (**16**) and Kleitman (**18**) have discussed in detail the physiological basis of sleep. When the scales are tipped in the opposite direction by prolonged rest, the refreshed organism requires activity to restore equilibrium. Thus cyclic changes in 'spontaneous' activity dovetail with cyclic changes in rest. This dual cycle of energy-transformation is not exclusively determined by internal factors but may be directly interfered with by environmental stimulation or its absence. The control of these drives is therefore partly external.

With respect to the mating drive, differences already noted in the male and female reproductive mechanisms require a different classification for the two sexes. The variation of the strength of the drive with the œstrous cycle in the female mammal marks it as definitely vegetative. The absence of a corresponding cycle in the male drive places

greater emphasis on external stimuli and thus shifts it closer to the middle of the scale.

Well over toward the other end we place the emergency drives of attack and escape, with their corresponding attitudes of ascendance and submission. Their dependence on external stimuli is clear. That they vary with internal conditions, though less obvious, is also indicated. Direct evidence is hard to find but indirect evidence is available. Certain chemicals influence the readiness with which the emergency drives are elicited. The increase of pugnacity so commonly attributed to alcohol receives an interesting commentary in the recent experiments of Gantt (**11**) on dogs, in which, under large doses of alcohol, two aggressive dogs showed withdrawal behavior while one timid dog behaved aggressively. Injections of adrenalin were reported by Marañon (**21**) to enhance the emotional accompaniments of directed recall, and Cantril (**6**) has recently confirmed the intensifying effect of adrenalin on fear reactions. A hyperthyroid condition frequently entails an increased disposition to worry and anxiety. The relation found by Stratton (**24**) between 'anger scores' and incidence of disease is suggestive, though its interpretation is far from clear. Although these conditions are too artificial or abnormal to be taken at face value, they suggest that milder alterations in blood chemistry may exert some effect.

The contention that every drive is influenced, to a greater or less degree, by both internal and external factors becomes obvious if we take account of the fact that no drive functions in isolation. The drives to food, rest, and mate may be inhibited by hostile intrusion, parallel with the inhibition of cranio-sacral by sympathetic innervation. Emergency reactions in turn are either opposed or facilitated by vegetative drives already in progress. The increased irritability noted, for example, by Landis (**19**) in an experiment involving sleep deprivation and fasting suggests that the threshold to emergency behavior varies within the normal activity and food cycles. More specifically, the blocking of a vegetative drive by an external agent is probably one of the most effective

stimuli to rage. However, the interdependence of simultaneous goal-activities is a complicated problem, which may well be preceded by a study of relatively isolated drives.

Classification of the exploratory drive is made especially difficult by its close relation to the activity-rest cycle. If it has any independent existence it lies in the instigation by external objects of approaching, investigating, or manipulating behavior. Strictly, then, it should be placed at the external end of the scale. Study of the interrelationships of drives would reveal its dependence on the internal conditions driving the organism to or from activity. Similar considerations probably apply to other so-called 'secondary' drives, for which no bodily mechanism has been discovered. If they depend on a brain organized to respond differentially to the environment, they are almost by definition externally aroused. This implies that the present criterion is chiefly useful as applied to the primary drives.

The question of the interdependence of drives brings up a related problem, but one not to be confused with it; namely, the use of the behavior characteristic of one drive to satisfy another. In the infra-human primates frequent cases of sex behavior in the service of escape and dominance have been observed by Hamilton (**12**), Tinklepaugh (**25**), and Maslow (**23**). This fact makes it clear that drives can not always be identified by the overt behavior involved, but must be interpreted both in the light of the external situation and the internal physiological condition.

V. Maturation and Learning

So far no attention has been paid to a fundamental problem in motivation; namely, the rôle of learning. We are faced with the question of a possible effect on the proposed classification through the conditioning of vegetative drives to external stimuli. As Carmichael (**8**, p. 460) suggests, citing Tracy and Coghill, "some organisms may be considered as at first driven blindly . . . by internal change" and "secondarily . . . the internally aroused activity . . . comes to be directed by exteroceptive stimulation." It is doubtful whether this shift of vegetative drives toward the

middle of the scale would radically upset their *relative* positions. For the sake of uniformity, however, these positions should be ascertained *as determined by maturation.*

But how can the rôle of maturation be distinguished? A recent viewpoint in psychology, accepting Stern's convergence theory as a way out of the instinct controversy, holds that learning and maturation are indissoluble (7). Marquis (22) attempted to preserve the distinction by locating the immediate stimuli to learning in the external environment and to maturation in the internal. There is some question, however, whether the essential modifications in learning take place 'in response to specific stimuli present in the external environment at the time of the modification' (22, p. 347), or whether they occur later. It is quite possible that the intimate physico-chemical changes are similar in both processes, as suggested by the work of Kappers (17). But at the behavior level a more valid distinction can be drawn. Here learning may be defined as *modification of the response to a situation due to previous response to a similar situation*, and maturation as modification independent of such previous response. To apply our criterion to the classification of drives, then, we must determine their relative susceptibility to internal and external stimuli when applied to the organism for the first time.

A further difficulty in distinguishing between learning and maturation has been most clearly formulated by Howells (14). He points out that the full influence of heredity can be revealed only by the relative ease with which different reactions can be learned. This implies a factor in development which might be called 'latent maturation' and which appears, not in unlearned behavior, but in a difference among *learned* behaviors. For a final understanding of the genetics of drive behavior the differential effects of maturation on learning would have to be investigated. But again it is doubtful if the basic classification of drives would be seriously altered thereby. The maturation here referred to, then, is explicit rather than latent, in the sense that it leads to a given response in the absence of previous learning.

As to the effects of learning on drives, they constitute a problem for research at least as challenging as the effects of maturation.

VI. Conclusion

An attempt has been made in the foregoing discussion to point out some of the basic factors in motivation. Many of these factors have been implicit in recent contributions to the subject, particularly those of Holt and Tolman; in the present paper they are made more explicit. It is hoped that this will make for a more systematic approach to the study of drives. A thorough working out of the relationships here suggested calls for further research along the following lines:

1. Determination of the adequate stimuli to the basic drives prior to learning. This would necessitate the application of a variety of stimuli at different stages of development. Hormone injections offer a valuable means of investigating the potency of internal factors.

2. Determination of the interactions of drives on one another.

3. Determination of the readiness with which different stimuli may be substituted for the originally adequate ones. In so far as the opportunities for learning are kept constant, this is the program advocated by Howells in order to reveal the rôle of heredity. When these are systematically varied as experimental factors, we shall be able to trace in some measure the contributions of biological drives to more complex purposes.

BIBLIOGRAPHY

1. Barcroft, J., Features in the architecture of physiological function, Cambridge Univ. Press, 1934, x + 368.
2. Bernard, L. L., Instinct; a study in social psychology, New York: Holt, 1924, p. 550.
3. Blatz, W. E., The physiological appetites, in 'A Handbook of Child Psychology' (2d ed.), (Ed., C. Murchison), Worcester, Mass.: Clark Univ. Press, 1933, pp. 723–770.
4. Cannon, W. B., Hunger and thirst, in 'A Handbook of General Experimental Psychology,' (Ed., C. Murchison), Worcester, Mass.: Clark Univ. Press, 1934, pp. 247–263.
5. ——, The wisdom of the body, New York: Norton, 1932, p. 312.
6. Cantril, H., The rôles of the situation and adrenalin in the induction of emotion, *Amer. J. Psychol.*, 1934, **46**, 568–579.

7. CARMICHAEL, L., Heredity and environment: are they antithetical?, *J. Abn. & Soc. Psychol.*, 1925, **20**, 245–260.
8. ——, A reëvaluation of the concepts of maturation and learning as applied to the early development of behavior, PSYCHOL. REV., 1936, **43**, 450–470.
9. CRAIG, W., Appetites and aversions as constituents of instincts, *Biol. Bull.*, 1918, **34**, 91–107.
10. ELLIOTT, M. H., AND BOUSFIELD, W. A., Two basic mechanisms in motivation, PSYCHOL. REV., 1936, **43**, 94–99.
11. GANTT, W. H., Effect of alcohol on cortical and subcortical activity measured by the conditioned reflex method, *Johns Hopkins Hosp. Bull.*, 1935, **56**, 61–83.
12. HAMILTON, G. V., A study of sexual tendencies in monkeys and baboons, *J. Animal Behav.*, 1914, **4**, 295–318.
13. HOLT, E. B., Animal drive and the learning process, New York: Holt, 1931, xii + 307.
14. HOWELLS, T. H., Heredity as a differential element in behavior, *Univ. Colo. Stud.*, 1933, **20**, 173–193.
15. JENNINGS, H. S., Behavior of the lower organisms, New York: Columbia Univ. Press, 1923, xvi + 366.
16. JOHNSON, H. M., SWAN, T. H., AND WEIGAND, G. E., Sleep, *Psychol. Bull.*, 1926, **23**, 482–503.
17. KAPPERS, C. U., On structural laws in the nervous system: the principles of neurobiotaxis, *Brain*, 1921, **44**, 125–149.
18. KLEITMAN, N., Sleep, *Physiol. Revs.*, 1929, **36**, 242–253.
19. LANDIS, C., Studies of emotional reactions: V. Severe emotional upset, *J. Comp. Psychol.*, 1926, **6**, 221–242.
20. LUDLUM, S. D., AND MCDONALD, E., The mechanism of disease, *Med. J. & Rec.*, 1925, **121**, 589–593.
21. MARAÑON, G., Contribution à l'étude de l'action émotive de l'adrenaline, *Rev. fr. d'endocrinol.*, 1924, **2**, 301–325.
22. MARQUIS, D. G., The criterion of innate behavior, PSYCHOL. REV., 1930, **37**, 334–349.
23. MASLOW, A. H., The rôle of dominance in the social and sexual behavior of infra-human primates: III. A theory of sexual behavior of infra-human primates, *J. Genet. Psychol.*, 1936, **48**, 310–338.
24. STRATTON, G. M., Emotion and the incidence of disease, PSYCHOL. REV., 1929, **36**, 242–253.
25. TINKLEPAUGH, O. L., Le comportement sexuel chez les chimpanzes et les singes inférieurs, considéré comme une réaction de substitution consecutive à des troubles emotionnels, *J. de Psychol.*, 1933, **30**, 930–954.
26. TOLMAN, E. C., Purposive behavior in animals and men, New York: Century, 1932, xiv + 463.
27. ——, Psychology versus immediate experience, *Phil. Sci.*, 1935, **2**, 356–380.
28. WARNER, L. H., A study of sex behavior in the white rat by means of the obstruction method, *Comp. Psychol. Monog.*, 1927, **4**, No. 22, 68.
29. WOODWORTH, R. S., Dynamic psychology, Columbia Univ Press, 1918, p. 210.
30. ——, Psychology (Revised ed.), New York: Holt, 1929, xiii + 590.
31. YOUNG, P. T., Motivation of behavior, New York: John Wiley, 1936, xviii + 562.

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